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TRANSACTIONS

OF A

SOCIETY

FOR THE

IMPROVEMENT

OF

MEDICAL AND CHIRURGICAL

KNOWLEDGE.

ILLUSTRATED WITH

COPPER-PLATES.

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G.R. III
BRITISH MUSEUM

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Blank Schemes for taking Caſes according to Dr. Fordyce's Plan, ſimilar to the two Sheets at p. 289, *may be had at Mr.* JOHNSON'*s, St. Paul's Church-yard. Price* 1 *s.*

TRANS-

TRANSACTIONS, &c.

I. *Obſervations on the Small-Pox, and the Cauſes of Fever.* By GEORGE FORDYCE, M. D. F. R. S. *Senior Phyſician to St. Thomas's Hoſpital, and Fellow of the Royal College of Phyſicians in London. Read December* 5, 1783.

NINE years ago *, I inoculated a family conſiſting of three young ladies, for the ſmall-pox. It was my cuſtom, at that time, to make three ſmall punctures in one arm. It happened in the eldeſt, that the ſuppuration in one of theſe, came much forwarder than in the other two: it was perfect on the ſeventh day, and the fever took place. I expected a great increaſe of the fever, which was already conſiderable, when the other two punctures ſhould perfect their ſuppuration, and was not a little alarmed for the ſafety of my patient: but was greatly ſurprized to find, when the firſt of theſe perfected its ſuppuration on the beginning of the eighth day, that it produced no effect whatever on the fever; neither did the ſecond, which perfected its ſuppuration on the end of the eighth.

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* This paper was printed in the year 1778, but not publiſhed.

The reaſon of making more than one puncture, was to enſure the diſeaſe's taking place, as it was ſuppoſed that a ſecond inoculation could not be made with ſafety, till it ſhould be certainly known that the firſt had not taken place, which is not till eight or ten days in many caſes, a time of dreadful anxiety to parents. I conceived now that this opinion was erroneous, and accordingly made no more than one puncture after that time.

It was not till the year after, that a caſe happened, in which I was in any doubt of the taking of the firſt puncture; and that was in a young gentleman, in his ſeventh year. On the third day after the firſt puncture, I made a ſecond. They both came forward. The fever was produced by the ſuppuration of the firſt, was very ſlight, and when the ſecond ſuppurated, was not at all increaſed; and there were not twenty puſtules in all.

It happened in about a month afterwards, that I was employed to inoculate a boy of four years. His parents ſent him a little way into the country, ſuppoſing he would be ſafer in a purer air. In viſiting him, the day after the eruption had appeared, I found a child of the woman who kept the houſe playing with my patient. I aſked the mother if he had had the ſmall-pox; was told No, but that he was preparing for inoculation, which was to be performed in two or three days. It occurred to me, if the natural infection had taken place, that it might be prevented by immediate inoculation, as I had grounds to believe, that it was generally from twelve to fourteen days after receiving the infection, that the natural ſmall-pox took place. It was impoſſible to meet the other practitioner till the next day. I therefore perſuaded the mother to let me inoculate the child immediately; ſhe inſiſting, however, that I ſhould not mention it; which, as I conceived the life of the boy was at ſtake, and there was no pecuniary conſideration, I conſented to. Three days after, the child was inoculated a ſecond time; my puncture being ſo ſlight, as not to be perceived. It inflamed however; the ſuppuration was perfected on the eighth day, the fever took place, and the eruption was beginning. The wound made afterwards, ſuppurated; but no new fever aroſe, and the patient had about fifty puſtules.

I have now ſo often repeated this practice, that I have no doubt, but that variolous matter has loſt all its power of producing fever, after the firſt twenty-four hours, from the time it is carried into the blood veſſels. I wiſhed, however, to try the effect of inoculation upon a perſon who had gone through the diſeaſe, and accordingly inoculated myſelf on the back of the hand, by a ſlight puncture. It inflamed in the uſual way, and a puſtule aroſe; ſo that I was under ſome apprehenſion of having the diſeaſe, eſpecially as I had already had two eruptive diſeaſes, which my phyſicians believed at the time were the ſmall-pox, inſomuch, as after the firſt, no care was taken to avoid expoſing me to the infection. I ſhould believe, that they were miſtaken in the firſt. On the fifth day, when the puſtule had ariſen to the ſize of the head of a large pin, it was hard, and without any matter, and gradually went away without ſuppuration. I have inoculated myſelf ſeveral times ſince. Sometimes the ſame kind of puſtule has taken place, and ſometimes there has been no inflammation at all.

Dr. Rutherford, my learned maſter, in his lectures on the practice of phyſic, to aſcertain the time the natural infection of the ſmall-pox is latent, before it produces the fever, mentioned the caſe of a party of ſoldiers marching through a village where the diſeaſe was, that they were ſeized from twelve to fourteen days afterwards with the fever, and that this was the uſual time. I have ſeen many caſes ſince, which confirm this opinion, although it alſo happens, that the infection ſometimes takes place ſooner. I therefore thought, that, if inoculation rendered the ſyſtem callous, if I may ſo ſpeak, to variolous matter, inſtead of there being any danger in inoculating a patient who had already caught the diſeaſe, as the infection was quicker in its action, the inoculation had a great chance of preventing any miſchief, by producing the inoculated inſtead of the natural diſeaſe. And this explained a circumſtance, which had before ſurprized and aſtoniſhed me, viz. the fool-hardineſs of certain empyrics, who brought a perſon to be inoculated into the room of a patient, when the ſmall-pox were at the height, as in many inſtances he muſt catch the natural ſmall-pox at the ſame time; but the inoculation coming before the natural kind, prevented the miſchief. It is nevertheleſs a dangerous practice; for, if the inoculation ſhould not take place, and the patient be infected, there is all the riſk of the natural ſmall-pox.

Since the time of my forming theſe opinions, I have known many inſtances which confirm them; ſo that I have no doubt in recommending it to any perſon, who may chance to have been expoſed to the natural infection, to be inoculated immediately.

To aſcertain the cauſe of the ſmall-pox being more or leſs numerous, has been a matter much wiſhed for. In the family I have already mentioned, the lady in whom one puncture ſuppurated before the others, and alone operated in producing the diſeaſe, the ſmall-pox were much more favourable than in the other two, where all the punctures ſuppurated together. This led me to reflect on the caſe of a girl of five years of age, whom I had inoculated about a month before. The mother of this child was exceedingly anxious, would hold the child herſelf, and ſtarted in ſuch manner, that the two firſt punctures hardly drew blood, and I was induced to make three more; five in all. This child had the diſeaſe very ſeverely, which I conſidered at that time as accidental only, but now began to think, that the quantity of matter abſorbed, and acting ſo as firſt to produce the fever, was the circumſtance which rendered the diſeaſe more or leſs favourable. I therefore reſolved to try a very ſmall puncture for the future, which has ſucceeded ſo well, that although I have inoculated a great number ſince that time, I have not met with one caſe in the ſmalleſt degree unfavourable; whereas, when I made a wound as I did at firſt, and applied the matter on a pledget, the diſeaſe was often unfavourable, and the wound very troubleſome, which made me change my method to making punctures, of which I made three, rather deep ones, and ſtill I met with many unfavourable caſes, ſo as always to inoculate with terror. I apprehend then, that the principal, if not the only conſideration in inoculation, is the manner of making the puncture, which ſhould be done as follows. Run a lancet into a puſtule, either at the time of ſuppuration, or before, ſo as it be moiſtened with the matter; let it dry; moiſten it a ſecond time; let it dry; view the point with a magnifier; if it be covered to the very point, it is properly armed; if it be not, it muſt be dipped in the matter again, until the point is covered compleatly; juſt before the puncture is to be made, dip the point in water for a ſecond; let it lie for a minute or two, till the matter be ſoftened again; lay bare the arm, and about the inſertion of the deltoid muſcle make a puncture, penetrating the ſcarf ſkin, ſo that it may be felt on raiſing the point of the lancet: if no blood appears, the better. Or if you have freſh matter in a quill, dip the point of the lancet in it, ſo that the point be perfectly covered, and proceed to make the puncture as before. Or if you have dry matter in a quill, moiſten it with a very ſmall quantity of water, ſo as to bring it to the conſiſtence of recent matter, and then proceed as before directed. It is not neceſſary to apply any thing to the arm;

after three days examine the arm; if there be no redneſs, make another puncture about an inch diſtance: this, however, will very rarely be found neceſſary.

Thus, I flatter myſelf, I have found what is moſt eſſential in inoculation. Preparation, it is true, has been ſuppoſed to be of great conſequence: but I believe this opinion is only a taint of that ſuperſtition, which originated with medicine, from the firſt practitioners, in the leſs cultivated ſtate of mankind, being prieſts and conjurers, as well as phyſicians; and I am afraid we ſhall never be able to get clear of this taint, which is the foundation of quackery. Preparation is a great inſtrument of ſuperſtition. A man is prepared to die, by rubbing a little oil on his breaſt; turning a chair three times round, is an excellent preparation for playing at whiſt: ſo eating ſoup for a month, renders the body perfectly prepared for mercury.

Taking the matter of preparation ſeriouſly, the propoſition is, to put the body into a ſtate, in which matter of the ſmall-pox ſhall produce leſs fever, fewer puſtules, or ſo that the puſtules ſhall have leſs effect on the ſyſtem. Now unleſs this ſtate be known, how ſhall we know in what manner it is to be produced? Will any one ſay, that he knows, on producing a perſon to him, who is to be affected with the ſmall-pox, whether he will have them favourably, or not? Certainly he cannot, on any principle hitherto known. I have had opportunities frequently, in St. Thomas's Hoſpital, of ſeeing perſons uſing mercurial, antimonial, and other regimens, recommended as preparations, ſeized with the ſmall-pox, which were in no ways more favourable, than in thoſe who were not uſing any of theſe remedies. The only thing that ſeems to have been of any advantage, is living for a little time on vegetable food; and this is even doubtful. I have the greateſt reaſon to believe, that it is not of the ſmalleſt conſequence, whether the matter be of the mild or confluent kind. I never knew of an inſtance of any other diſeaſe being communicated by inoculation of the ſmall-pox.

There is therefore only one other circumſtance, excepting the puncture, to be attended to; which is, that, of the children who have died in London of inoculation for many years, as far as I have been able to collect, more than two thirds have been under nine months; and therefore I have conſidered it as improper to inoculate before teething. It happened to me, that a family came from America, whoſe children had not had the ſmall-pox. One of theſe was under nine months, and on the breaſt. They applied to me. I refuſed to inoculate this child, and adviſed that it ſhould be kept out of the way while the others were inoculated; but rather than ſubmit to this inconvenience, they applied to another practitioner, who inoculated the whole, and this child died.

Since freſh variolous matter has no effect on the fever in the ſmall-pox, it follows, that that which is already in the blood-veſſels has alſo no further effect, after the diſeaſe is perfectly brought on; and as other fevers are ſo exactly ſimilar to that of the ſmall-pox, that it is impoſſible to diſtinguiſh them till the eruption takes place, it is very probable, that other infections, as well as the variolous, only act for a ſhort time in producing the fever, and that They alſo have no further effect. That this is the caſe, we have greater reaſon to believe, from this obſervation, that patients who have caught infectious fevers in St. Thomas's, and other Hoſpitals, and continued there, have gone through the diſeaſe in a ward where the infection was ſtrong, with as mild ſymptoms, as where peculiar care has been taken to keep the air as pure as poſſible. And in general many more people recover of fevers in our Hoſpitals than in private families, with ſimilar practice; owing moſt probably to the directions of the practitioners being more ſtrictly attended to; for ſuch is the diſpoſition of mankind to quackery, that you can hardly ever prevent, even a fond mother, from trying experiments on her only ſon, lying ill of a dangerous diſeaſe.

Moreover, if a criſis ſhould take place in the middle of an infectious fever, it happens when the infection is ſtrongeſt, and its effects therefore the moſt powerful, if it acts at all on the patient.

Again, thoſe who have once gone through the fever, are leſs ſubject to the infection, than thoſe who have not had the diſeaſe. This has been long remarked in the plague. I have obſerved it in our Hoſpital, in which there are almoſt always violent fevers, and therefore infectious ones; for all fevers, from whatever cauſe they ariſe, I have obſerved, generate infectious vapour, and commonly according to their violence. Another ſtrong inſtance is, that ſome of the felons, who have brought infection into a court of juſtice, ſo as to deſtroy almoſt half the court, have had no fever at the time. In the laſt caſe which happened at the Old Bailey, the felon was not at the time affected with fever, or any other diſeaſe.

From all theſe circumſtances we may conclude, that during the courſe of a fever, which aroſe from infection, the infectious matter has nothing to do with the fever, neither increaſing, nor diminiſhing, nor any ways altering it; and that therefore it is not neceſſary to pay any attention to it in the cure of the diſeaſe.

It is worth while enquiring, how far other cauſes of fever have effect upon it, after it has once taken place. Expoſure to cold or moiſture, or to anxiety, are two other cauſes which I have known produce fever. I have ſeen ſeveral caſes, in which theſe have undoubtedly brought on the diſeaſe, which has ariſen immediately on their application. Where the anxiety is gone off, the fever has nevertheleſs continued; and when the patient has been brought into a warm and dry air, the fever has continued, and gone through its natural courſe; ſo that it is only neceſſary, that theſe cauſes of fever ſhould be once applied, ſo as to make an impreſſion. The diſeaſe being once produced, goes on of itſelf; nevertheleſs, both anxiety, and cold or moiſture, have an effect in keeping up fever. When intermittents cannot be cured in fenny countries, they give way eaſily on the patient's coming into dry air; as I have obſerved in patients brought from Lincolnſhire to the Hoſpital, who have been eaſily cured, although the ſame remedies have been uſed before without ſucceſs. And I have, in ſeveral inſtances, ſeen continued fevers ſpun out to four, five, ſix weeks, in conſequence of diſtreſs of the mind.

Why violent fevers ſhould have been called putrid ones, is very difficult to gueſs, as frequently we find them paſs through their courſe without any ſign of putrefaction. There are no purple ſpots or marbling, no looſeneſs of the gums, no blackneſs of the tongue, no hemorrhage, no fetor in the excretions except the fæces, no fetor in the breath, no frothineſs in the urine, in a great many of them when they are highly infectious; ſo that infectious vapour is a matter totally different from the vapour which ariſes from putrid ſubſtances, which, however, is alſo certainly a cauſe of fever, and which has been ſuppoſed to act by being introduced into the blood-veſſels, and producing putrefaction of the fluids. Now it may be queſtioned, whether putrid vapour does not alſo act *uno ictu*, in the ſame manner as the cauſes we have already enumerated. That it does not act by inducing putrefaction of the fluids is rendered probable, in as much as this ſtate of them in a fever depends evidently on the depreſſion of ſtrength, and may accordingly be foreſeen. If at once the muſcular ſtrength be greatly depreſſed; if there be great ſenſe of weight about the precordia, with ſighing; if there be great ſtupidity in the eye, and want of attention and general diſtreſs of the mind; we may expect ſymptoms of putrefaction to appear, whatever might be the cauſe of the fever.

Moreover, theſe ſymptoms of putrefaction when they appear are always the conſequence of the fever; for it is ſeveral days after the fever takes place, before they make their appearance.

Again, I have known them go off entirely when a phlegmon has ariſen, and the fever ſtill continued. This happened in two caſes. In leſs than twelve hours, the pulſe became hard and ſtrong, inſtead of having that languor with which it beat before. And ſometimes in fevers ariſing from putrid vapour, no appearance of putrefaction takes place, and the fluids do not become putrid more readily in theſe with the ſame depreſſion of ſtrength, than in others; and the progreſs of the fever is ſeldom different in conſequence of theſe ſymptoms of putrefaction; ſo that I ſuſpect that a fever brought on by putrid vapour, is alſo independent of its cauſe, and would go on, when once produced, if there were no longer the leaſt particle of putrid matter in the body.

Food of difficult digeſtion, I have alſo known evidently produce a fever, and very often occaſion relapſes of fevers. Theſe fevers go on when the primæ viæ have been cleared of all offending matter, and therefore are independent of their cauſe. All fevers, however, are much increaſed by improper food, from whatever cauſe they ariſe.

Upon the whole, therefore, it would appear, that a fever produced from any of the known cauſes goes on, notwithſtanding the cauſe be totally removed; and that the action of infectious, and probably putrid vapours, in bringing on a fever, renders the body incapable of being farther affected by theſe vapours, in ſome caſes, for a ſhort time only, and in others for the whole life-time of the patient; and that therefore it ſeldom is of uſe, to employ any remedy to remove the cauſe of the diſeaſe after the fever has actually taken place.

II. *Obſervations on the Inflammation of the internal Coats of Veins.* By JOHN HUNTER, Eſq. F. R. S. *Surgeon Extraordinary to His* MAJESTY, *and Surgeon-general to the Army. Read Feb. 6, 1784.*

THE following obſervations will ſhew that the inſide of veins, as well as of all other cavities, is a ſeat of inflammation and abſceſs. I have found in all violent inflammations of the cellular membrane, whether ſpontaneous or in conſequence of accident, as in compound fractures, or of ſurgical operation, as in the removal of an extremity, that the coats of the larger veins, paſſing through the inflamed part, become alſo conſiderably inflamed; and that their inner ſurfaces take on the adheſive, ſuppurative, and ulcerative inflammations: for in ſuch inflammations I have found in many places of the veins adheſion, in others matter, and in others ulceration. Under ſuch circumſtances, the veins would have abſceſſes formed in them, if the matter did not find in many caſes an eaſy paſſage to the heart along with the circulating blood, ſo as to prevent the accumulation of the pus: but this ready paſſage of the matter into the common circulation, does not always happen. It is in ſome caſes prevented by the adheſive inflammation taking place in the vein between the place of ſuppuration and the heart, ſo that an abſceſs is formed, as will be further obſerved.

Where the inflammation is moſt violent, there we find the vein moſt inflamed; there alſo, after ſuppuration, we find the pureſt pus; and as we trace the veſſels from this part, either farther from or nearer to the heart, we find the pus more and more mixed with blood, and having more of the coagulated parts of the blood in it.

As theſe appearances are only to be ſeen in dead bodies, they cannot be deſcribed but from thence; but it is ſo common a caſe, that I have hardly ever ſeen an inſtance of ſuppuration in any part, furniſhed with large veins, where theſe appearances are not evident after death. I have found them in the bodies of thoſe who have died from amputations, compound fractures, and mortifications.

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Theſe circumſtances all conſidered lead us to account for a very frequent complaint, that is, an inflamed arm after bleeding; a complaint which has by ſome been imputed to the wounding of a tendon; becauſe the tendon of the biceps muſcle lies under ſome of thoſe veins, in which we often bleed, and when the complaint occurs, it is unjuſtly ſuppoſed to ariſe from want of ſkill in the operator; by others it has been ſuppoſed to ariſe from the wounding of a nerve; and again it has been laid to the charge of a bad conſtitution.

But if we conſider more critically this conſequence ariſing from bleeding, we ſhall find that it happens frequently after bleeding in veins where no tendon could poſſibly be wounded, and alſo where no particular nerve could be in the way. It ſeems likewiſe to happen as frequently in conſtitutions where there is no appearance of want of health, as in thoſe diſpoſed to diſeaſe. As a proof of this laſt, upon bleeding in another vein in the ſame perſon, perhaps with a view to aſſiſt in the cure of the inflammation ariſing from the firſt bleeding, the wound has healed very readily.

If we examine the proportion which the number of thoſe inflammations that happen after bleeding, bear to thoſe which ariſe from as ſlight a wound where no large vein has been injured, and even perhaps where the wound has not been made by ſo clean a cutting inſtrument, and the ſame pains not taken to cloſe it up, we ſhall find that thoſe from wounded veins are much the moſt frequent; and that ſuch inflammations ſeldom or ever happen under the laſt mentioned circumſtances; therefore, we muſt look for ſome other cauſe to explain this effect of bleeding.

The manner in which thoſe ſore arms come on, ſhows plainly that they ariſe from the wound not healing by the firſt intention; for the external wound, in moſt caſes, firſt feſters or inflames, then ſuppurates and ulcerates, ſo that the cavity of the vein becomes impervious. In ſome this ſuppuration is only ſuperficial, the vein and parts below having united. In others the ſkin ſhall appear to be united, but not cloſe to the vein, ſo that a ſmall abſceſs ſhall form between the ſkin and the vein; it ſhall burſt and diſcharge a thin watery fluid, and no further miſchief happen;

but

but when this imperfection of union is continued on to the cavity of the vein, then the vein inflames both upwards and downwards, and that often for a conſiderable way, and the ſurrounding parts join in the inflammation.

We find all theſe variations in different caſes; for the diſeaſe ſometimes goes no further than an inflammation in the vein near to the orifice, which is often reſolved; at other times the inflammation is carried further, but ſuppuration is prevented by the adheſive inflammation taking place in the vein at this part, ſo as to exclude the ſuppurative inflammation, and the veins in ſuch caſes may be plainly felt after the ſurrounding tumefaction has ſubſided, like hard cords. But this ſalutary effect is not always produced, and ſuppuration in the vein is the conſequence, but often ſo confined, that only a ſmall abſceſs forms in the cavity of the vein near to the orifice. The confinement of the matter in this part of the vein, ariſes from adheſions in the vein a little above and below the orifice. But in many caſes the inflammation and ſuppuration are not confined to this part from the adheſions not having taken place; for it frequently happens that an abſceſs is formed, occupying a conſiderable length of the vein both ways; and we often have more than one abſceſs, nay at times there is a ſeries of them, and generally in the direction of the vein, between the orifice and the heart; but not always in this courſe, for we find them ſometimes between the wound and the extreme parts.

I have ſeen from a wound in the foot the vena ſaphæna inflamed all up the leg and thigh, nearly as high as the groin; and I have been obliged to open a ſtring of abſceſſes almoſt through its whole courſe.

In caſes where I have had opportunities of inſpecting veins after death, in which the inflammation had been violent, upon examining the vein at ſome diſtance from this violence, I found the inflammation in the adheſive ſtate: in ſome places the ſides of the vein were adhering, and in others the inner ſurface of the veins was furred over with coagulable lymph. Where different abſceſſes had formed, I have always found that the ſpaces of the vein between them had united by the adheſive inflammation, and it is this union which circumſcribes the abſceſſes.

Upon examining the arm of a man who had died at St. George's Hoſpital, I found the veins, both below and above the orifice, in many places united by the adheſive inflammation. I alſo found in many parts of the veins, that ſuppuration had begun as we find on an inflamed ſurface, but had not yet arrived at ulceration; and in ſeveral other places ulceration had taken place, ſo as to have deſtroyed that ſurface next the ſkin, and a circumſcribed abſceſs was formed. The vein near to the axilla had taken on ſuppuration, beyond which adheſions had not formed, and this had given a free paſſage for the matter into the circulation, of which moſt probably the patient died.

In thoſe caſes where larger abſceſſes have come on than thoſe formed ſimply from the ulceration of the wound made by the lancet, I have always found that the vein was afterwards obliterated, having united and healed up as any other cavity does, ſo that ſuch patients could never be bled in the ſame vein again, which is a proof that the ſides of the vein can unite by the adheſive inflammation.

Inflammation of a vein is a common effect after bleeding horſes, which is uſually done in the neck. The operator on this animal does not always take ſufficient care to cloſe up the external wound; for although the method uſually employed would at firſt ſight appear to be a good one, that is, by a pin paſſed through the wound from ſide to ſide, as in the hare-lip, and over-tied by a thread or hair, yet, if not executed with ſufficient attention, I ſhould be inclined to believe that it is the very worſt, as it very readily promotes inflammation in the cavity of the vein, either of the adheſive or ſuppurative kind, according as the ligature does or does not communicate with the cavity.

In ſome of theſe inflammations of horſes I have ſeen the jugular vein inflamed through its whole length, and all the ſide of the head has been conſiderably ſwelled, and the inflammation carried along the vein quite into the cheſt. In theſe caſes there is always an abſceſs formed at the wound, and often ſeveral along the vein, as in the human ſubject; and whenever the complaint is carried ſo far as this ſtage, the cavity becomes united at thoſe places by granulations, and the vein is ever after impervious. Many horſes die of this diſeaſe, but what is the particular circumſtance which occaſions their death, I have not been able to determine. It may either be that the inflammation extends itſelf to the heart, or that the matter ſecreted from the inſide of the vein, paſſes along that tube in conſiderable quantity to the heart, and mixes with the blood.

I am inclined to believe that the expoſure of cavities of the larger veins in caſes of accidents, and alſo of operations, is often the cauſe of many of the very extenſive inflammations which ſometimes attend theſe caſes, and indeed may be the reaſon why inflammations extend or ſpread at all beyond the ſphere of continued ſympathy.

In all caſes where inflammation of veins runs high, or extends itſelf conſiderably, it is to be expected that the whole ſyſtem will be affected. For the moſt part the ſame kind of affection takes place which ariſes from other inflammations, with this exception, that where no adheſions of the ſides of the veins are formed, or where ſuch adheſions are incomplete, pus paſſing into the circulation may add to the general diſorder, and even render it fatal.

In all caſes of inflammation where adheſions take place, they ariſe from an extravaſation of coagulable lymph; but how ſuch adheſions ſhould take place on the internal ſurface of veins, appears at firſt ſight difficult to conceive, ſince it is moſt obvious, that the coagulable lymph thrown out by the exhalants on the internal ſurface of the vein, mixing with the ſame fluid circulating with the other parts of the blood, would be ſwept away without producing any effect. But ſince ſuch adheſions do in fact happen, the coagulable lymph muſt undergo ſome change connected with the diſpoſition which produces its extravaſation.

Although the operation which is the moſt frequent cauſe of this complaint is to appearance trifling, yet as it is often of very ſerious conſequence, both to the life of the patient and the character of the ſurgeon, it requires particular attention in the operator to prevent as much as poſſible an evil of ſuch magnitude. With this view he will be particularly attentive to the mode of cloſing the wound, and binding up the arm. This is to be done by bringing the two ſides of the wound together, that they may unite by the firſt intention.

tention. To accompliſh this, let the ſurgeon, with the thumb of that hand which holds the arm, puſh the ſkin towards the orifice, while he draws it on the other ſide to the ſame point with the compreſs; thus the ſkin will be thrown into folds at the wound over which he is immediately to apply the compreſs. The compreſs ſhould be broad, to keep the ſkin better together; and thick, to make the compreſſion more certain. Another advantage ariſing from this caution, is the prevention of the vein bleeding a ſecond time. I have known an inflammation attack the orifice, which appeared to have ariſen from the firſt union having been broke through, and no ſecond union formed; but this probably did not ariſe from the vein being opened a ſecond time, but from the ſides of the orifice not having been again brought together. I would recommend a compreſs of linen, or lint, in preference to ſticking plaſter; for I imagine that the blood drying over the orifice is a kind of bond of union more natural and effectual than any other application: and this concluſion is drawn from practice; for I have ſeen more ſore arms in conſequence of bleeding where plaſters have been afterwards applied, than from any other; and in caſes of the compound fracture, when attempted to be cured as a ſimple fracture, if the wound will allow of being ſcabbed over, I have ſeen it always do well; whereas if it has been kept moiſt, or prevented from evaporating by plaſters, or other applications, it has always ſuppurated.

When inflammation takes place beyond the orifice, ſo as to alarm the ſurgeon, he ſhould immediately make a compreſs upon the vein at the inflamed part, to make the two ſides adhere together; or if they do not adhere, yet ſimple contact will be ſufficient to prevent ſuppuration in this part; or if inflammation has gone ſo far as to make the ſurgeon ſuſpect that ſuppuration has taken place, then the compreſs muſt be put upon that part of the vein juſt above the ſuppuration. This I once practiſed, and, as I ſuppoſed, with ſucceſs.

III. *A Proceſs for preparing pure Emetic Tartar by Re-cryſtallization. By Mr.* Jenner, *Surgeon at Berkeley. In a Letter to* John Hunter, *Eſq. Read June* 4, 1784.

Sir,

I believe it is generally acknowledged, that no preparation of antimony obtains ſo much in the modern practice of phyſic as emetic tartar; yet how greatly ought it to be lamented, that a proceſs of ſuch conſequence to the health of mankind has hitherto been conducted upon an imperfect plan. In the uſe of this valuable medicine, the phyſician's warmeſt hopes are too frequently allayed with fear: for, while ſo many *formulæ* for the compoſition of emetic tartar are extant in diſpenſatories and chymical treatiſes, the practitioner, wanting a fixt criterion of its quality, muſt often preſcribe with uncertainty, and generally be diſappointed in his concluſions. Indeed the faculty are well aware that a ſingle grain prepared by one chymiſt may excite full vomiting, though the ſame quantity prepared by another may not even produce a nauſea. Theſe conſiderations, added to the great importance of the medicine itſelf, have induced me to attempt ſome experiments, with a view of bringing it to the greateſt degree of purity. The reſult of theſe proved favourable to my wiſhes; and without entering into a tedious detail of them, I flatter myſelf that emetic tartar made according to the incloſed proceſs, will bear the teſt of a ſcrupulous examination. It will be found to be a pure neutral ſalt, compoſed entirely of tartar and the reguline part of antimony, formed in cryſtals beautifully white and tranſparent, and perfectly ſoluble either in water or wine. I am,

Sir,

Your moſt obedient

and obliged ſervant,

Berkeley,
Feb. 5, 1784.

EDWARD JENNER.

The

The Proceſs for preparing pure Emetic Tartar by Re-cryſtallization.

Take cream of tartar and glaſs of antimony levigated, of each one pound. Mix them together, and pour upon them in a clean earthen veſſel two gallons of boiling water. Stir the whole together a few minutes, and the cream of tartar will be fully ſaturated. Let the fæces ſubſide, filter the liquor, and boil it immediately in a well-tinned veſſel, till the whole be nearly evaporated to dryneſs. Pour as much boiling water on the emetic tartar, thus rudely formed, as will diſſolve it; put it into an earthen veſſel, and let it remain twenty-four hours. Emetic tartar will now be formed in cryſtals adhering to the ſides and bottom of the veſſel, but not perfectly bright and free from impurities. Decant the liquor, and boil it till a pellicle appears on the ſurface. Set it by to cryſtallize. Collect theſe cryſtals, and mixing them with thoſe formed before, diſſolve them in as much boiling water as is ſufficient for that purpoſe. Filter the ſolution immediately; put it into clean glaſs veſſels, and let it remain

main twenty-four hours. The cryſtals which appear now are in a perfect ſtate. Having waſhed them with a little clean cold water, place them in the ſhade on white ſpongy paper to dry. To obtain the whole of the emetic tartar contained in the remaining liquor, the proceſſes above mentioned muſt be again and again repeated.

IV. *An Account of the Diſſection of a Man, that died of a Suppreſſion of Urine, produced by a Collection of Hydatids, between the Neck of the Bladder and Rectum; with Obſervations on the Manner in which Hydatids grow and multiply in the human Body. By* JOHN HUNTER, M. D. F. R. S. *and Phyſician to the Army. Read April* 17, 1787.

THOMAS BELL, aged forty-ſix years, a carpenter, and a ſtout man, died ſuddenly, March the 17th, 1786. The following particulars were all that could be learned reſpecting his diſeaſe previous to his death: that he had been complaining for four or five weeks of more or leſs pain and difficulty in making water, which he himſelf ſuppoſed to proceed from gravel, but by thoſe about him was ſuſpected to ariſe from ſome venereal complaint. In conſequence of this ſuſpicion he was viſited by a ſurgeon, who examined the *penis*, and found a natural *phimoſis*, but no venereal complaint. When queſtioned reſpecting his difficulty of making water, he ſaid, that he had been in much pain, but was now eaſier, and that ſome urine had come away involuntarily. He was able to ſit up in bed at this examination, but in an hour after, in attempting to turn himſelf, he expired.

The body was examined thirty hours after death.

Head.—The brain was in all reſpects natural; it was not firm, however, for ſo ſhort a time after death. The water in the ventricles was in the uſual quantity. The blood in the veſſels was fluid both in the head and other parts of the body.

Thorax. The viſcera were all ſound.

Abdomen. The belly was very tumid, which, on removing the common integuments, was found to proceed from the immenſe ſize of the bladder. It was diſtended enormouſly, and reached fully eight inches above the *pubis*; its fundus was within two inches of the arch of the *colon*. Upon letting out the water, which amounted to five or ſix pints, it appeared that there was a large tumour between the neck of the bladder and the *rectum*, which completely filled the pelvis, and thruſt the bladder forwards and upwards. On cutting into the tumour much water ruſhed out, and along with it many hydatids of various ſizes; the largeſt was about an inch and an half in diameter, and the ſmalleſt not larger than a pin's head. The tumour was intirely filled with hydatids and the water that ſurrounded them, and both together they were more in quantity than a pint and an half. There were beſides two or three ſmaller tumours near the neck of the bladder, alſo containing hydatids; and there were two bodies, not larger than common beans, adhering to the bladder, containing a ſoft cheeſe-like ſubſtance.

Between the ſtomach and the ſpleen, and over one end of the pancreas, there was a large tumour, to which the three above parts adhered; the ſtomach and pancreas ſlightly by cellular membrane; the ſpleen more intimately, ſo as to make a part of the tumour: with the ſpleen it was about ten inches in diameter. It was irregularly ſhaped, and made up of ſeveral ſmaller tumours. There was conſiderable variety in the contents of thoſe tumours; in one there were hydatids of various ſizes, like thoſe mentioned above; in another there was a ſubſtance like iſinglaſs, a little ſoftened in water; in a third there was clear

Plate 1, p.37

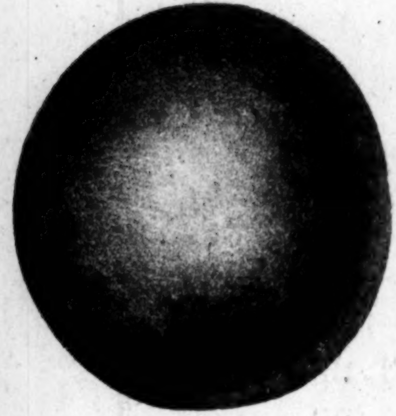

Fig. 1.

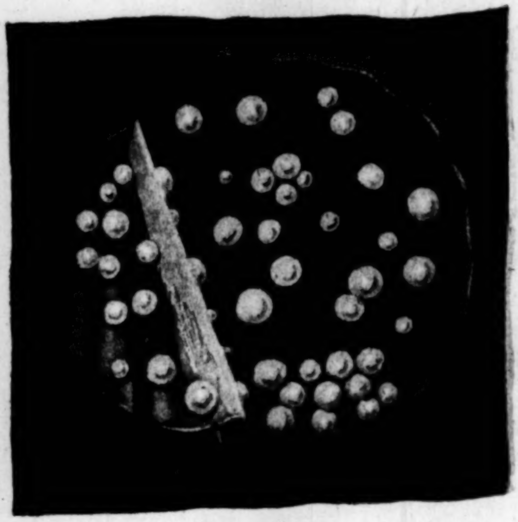

Fig. 2.

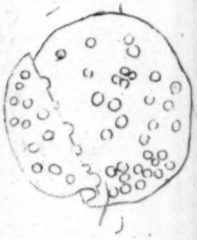

Fig. 3.

water in a conſiderable quantity, with very minute particles, like ſmall grains, adhering ſlightly to the ſides; and in a fourth there were hydatids, ſome full, others burſt, and with their coats compreſſed together, and forming the iſinglaſs-like ſubſtance. The tumours or ſacs had all thick coats, endowed with a ſtrong contractile power, that forcibly protruded their contents through any opening made into them. They had two coats; an outer, which was ſtrongeſt and thickeſt, and an inner, which was tender, ſoft, and pulpy.

As to the ſtructure of the hydatids, it was the ſame in large and ſmall; a tranſparent bag, uniformly round and ſmooth, filled with clear water. The bag appeared to conſiſt of two coats, or layers; for on handling them, the outer coat would get rumpled, and occaſion a degree of opacity, but, by wiping the hydatid, it became again clear and tranſparent. They appeared to be completely ſpherical, except that the large ones were a little flattened by their own weight, when laid on a plate *. They adhered no where to the ſides of the ſac, nor to one another. When they were opened, their coats poſſeſſed a ſtrong con-

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* Vid. Plate Iſt. Fig. 1.

contractile force, ſo as to roll themſelves up in part. On examining a number of hydatids, ſome of them appeared of an amber colour, and with thicker coats than the reſt; and when opened, their inner ſurface was found covered with ſmall hydatids, which were not ſo large as the heads of pins, and looked like minute pearls or ſtuds ſet in the inner coat *.

Some of the water containing the ſmall grains mentioned above, was examined with a microſcope, and found to have floating in it numerous minute hydatids; of which the largeſt were the little grains viſible to the naked eye, and $\frac{1}{200}$ part of an inch in diameter; the ſmalleſt were leſs than a red globule of blood; and they were of all intermediate ſizes. The coats of the largeſt were a little rough with numerous filaments, or *villi*; and, on uſing a deeper magnifier, they had ſomewhat of a mulberry appearance †.

When the young ones growing in the coats of the larger were examined with the microſcope, they were found not to be ſet in the coats, like pearls, but to be covered by a thin tranſparent membrane, ſo as to lie between two layers. It is not improbable that the ſmall globules attach themſelves by the *villi* to the ſide of the hydatid, and to each other, and thereby give the appearance of being covered by a thin membrane. However that may be, the globules being found of various ſizes floating in the liquor, ſeems to prove that they are originally formed there, and not in the coats of the hydatid, upon which they are afterwards depoſited. The number of thoſe that had young ones in them, was few in proportion to the others.

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* Vid. Plate Iſt. Fig. 2.

† Vid. Plate Iſt. Fig. 3.

The hydatids in their growth and decay appear to paſs through various ſtages; they are firſt found floating in the fluid that fills the hydatid, and afterwards attached to its coats. The hydatid thus pregnant with young, if the expreſſion may be allowed, adheres to the neighbouring parts, increaſes in ſize, and becomes itſelf a ſac, containing numerous ſmall hydatids. Theſe after a certain time decay, and the ſkins or empty bags are ſqueezed together into a ſubſtance like iſinglaſs. It is probable they ſtill undergo a further change; two ſmall bodies, of the ſize of the common bean, of a cheeſe-like conſiſtence, and covered with a ſkin, were taken notice of adhering to the bladder near its neck; it may be a queſtion whether thoſe were not the remains of hydatids? but that muſt be determined by future obſervations. It is to be obſerved, that the young hydatids are found in two very different ſtages; in the one they are attached to the coats of an hydatid, that floats looſe in the parent bag or ſac; in the other, extremely ſmall globules adhere ſlightly to the inner ſurface of a bag or ſac, which is firmly attached to the neighbouring parts, and covered with a ſtrong outer coat. It is obvious that the progreſs of growth is very unequal in thoſe two, and indeed inverted; for in the firſt the young ones are as large as the heads of pins, while the parent bag is not larger than a walnut, and floats unattached; but on the contrary, in the ſecond there is a large ſac with a ſtrong outer coat, and a more tender inner one, adhering ſtrongly to the ſurrounding parts, while the young ones, that are very ſlightly attached to its ſides, are not of a larger diameter than a $\frac{1}{200}$ part of an inch. Whether thoſe are merely accidental differences in the growth, or depend upon ſome

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ſome more eſſential diſtinction, muſt remain to be determined by future obſervations.

The term *hydatid* is uſed in different ſenſes; thus the hydatids of the *placenta*, of the *ovaria*, and of the kidneys, which may be conſidered as morbid changes in the ſubſtances of thoſe bodies, are totally different from the hydatids of the preſent caſe, which produce their like, and multiply in great abundance, without any further connection with the human body, except in ſo far as it affords them a nidus. The obſervations I am going to make are confined intirely to this laſt mentioned kind. They have been found in various parts of the human body; they have been diſcharged both by vomit and by ſtool *; they have been brought up from the lungs by cough †; they have been voided by urine; and they have been diſcharged from tumours in different parts of the body ‡. Le Cat has taken notice of their inner coat being covered with ſmall *mamillæ*, an appearance produced by the young ones, and ſuppoſes them to be the glandular grains of the ſpleen diſtended with lymph. They have alſo been conſidered as diſeaſed lymphatics; but it muſt be obvious, that the young ones growing within the larger, at the ſame time that theſe laſt are floating in a liquid without connection with the ſides of the ſac, cannot proceed from any diſeaſe in the lymphatic ſyſtem, if by that be underſtood the abſorbent veſſels. It is unneceſſary to animadvert upon Le Cat's opinion, as anatomy has not hitherto been able to demonſtrate the exiſtence of any glandular grains in the ſpleen, which appears to be a congeries of blood-veſſels. It may be a queſtion, whether the hydatids are not of an animal nature, and poſſeſſing a life peculiar to themſelves? We are not yet in poſſeſſion of a ſufficient number of facts to aſcertain this point; but the diſcoveries that have been made reſpecting the nature of hydatids in other animals, promiſe to throw ſome light upon the preſent ſubject.

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* Philoſoph. Tranſact. Vol. xxii. p. 1797.

† Med. Tranſact. Vol. ii. p. 486.

‡ Philoſoph. Tranſact. Vol. xxv. p. 2344.

The ingenious naturaliſt Pallas, in his Miſcellanea Zoologica *, has given an account of the obſervations made by others upon hydatids, and added diſcoveries of his own. There are only two before him, who knew the hydatids

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* Ed. Hagæ Comitum, 1766, p. 157.

datids in animals to be alive, and to poſſeſs a peculiar ſtructure and power of motion. Thoſe were Dr. Tyſon and Phil. Jac. Hartmannus. Pallas conſiders Tyſon as the firſt diſcoverer, but ſeemingly without reaſon, for Hartmannus publiſhed his account in the year 1685 *; and Tyſon in 1691 †. Neither of them appears to be acquainted with the other's diſcovery. Tyſon obſerved the neck and mouth of the animal, and ſaw them in motion; Hartmann further ſaw the whole body in motion by putting them in warm water. Pallas has examined them very minutely, and gives them the name of *tænia hydatigena*, from finding their heads of the ſame ſtructure as that of the *tænia*. But in all that Pallas ſaw he never found any pregnant, ſo that he was unacquainted with the manner in which they multiply. Goetze, a German writer, has given figures of the young ones adhering to the parent bag; and Mr. Hunter has preparations of that kind in his collection. The Abbé Fontana ‡ has obſerved them in ſheep, ſeen the motion of the young ones adhering to the ſides, and alſo with a microſcope examined the heads of them, and found them reſembling thoſe of *tæniæ*. So far the obſervations of authors go with reſpect to the hydatids of animals; in regard to the hydatids of the human ſpecies, Tyſon has remarked that they are different from the former; that they are uniformly round, without any appearance of a neck or head; but he declines giving any opinion of their nature. Pallas * likewiſe mentions a kind of hydatids found in the human and other bodies, without any neck or mouth, but leaves their nature to be decided by future obſervations. He ſays their coats are very elaſtic, and when cut retract with ſo much force as to roll themſelves up, turning the inſide out.

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* Miſcell. Nat. Cur. dec. 2. An. 4to.

† Phil. Tranſ. for that year.

‡ Opuſcoli Scelti, tom. 6.

It is not altogether allowable to conclude, from the reſemblance of the human hydatids to thoſe of quadrupeds, that the firſt as well as the laſt form a part of the animal kingdom; yet they agree in ſo many circumſtances, that ſuch a concluſion appears very probable. In quadrupeds they have their ſeat generally in the *abdomen*, and moſt commonly in the liver or ſpleen; the ſame is true of the human hydatids. In the human they multiply by the young growing on the inſide of the hydatids; and the ſame has been obſerved in quadrupeds. The analogy fails when we look for a mouth and neck in the human; nor has any periſtaltic, or undulating motion been obſerved in their coats; but this laſt appearance has not been looked for in a proper way, that is, by putting them in tepid water as ſoon as they come from the human body.

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* P. 172.

With regard to the time requiſite for the growth and decay of the different ſacs containing hydatids, it is difficult to form a conjecture. Our patient was able to follow his trade as a carpenter till five weeks before he died; it is probable therefore that in that time, or a little more, the ſac between the neck of the bladder and rectum had grown to that ſize, which produced a ſuppreſſion of urine and death. The growth and decay of the hydatids ſerve to explain the increaſe and diminution, which have been obſerved in tumours of the abdomen proceeding from this cauſe, as in the caſe mentioned in the Medical Tranſactions *. It will not be deemed out of place to mention ſome particulars of a diſſection that have come to

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* Vol. II.

my knowledge, that in a great meaſure explain the appearances in the caſe above alluded to, and confirm the truth of the conjecture ſubjoined to it.

A patient died in the Infirmary at Edinburgh, with the ſymptoms of the encyſted dropſy. His abdomen was ſwelled with many irregular protuberances. On examining the body, there were found numerous encyſted tumours full of hydatids. They had their baſis in the liver, from which they proceeded not only downwards into the abdomen, but upwards into the thorax, and making their way through the diaphragm, came in contact with the lungs. Had the patient lived a little longer, it is not improbable that a communication between the bags containing the hydatids and ſome of the branches of the *trachea arteria* would have taken place, which muſt have produced the ſame ſymptoms as in the caſe abovementioned.

To return to the diſſection. The hydatids did not appear to have done any injury to the parts, except by mechanical preſſure, for the viſcera were all uncommonly ſound. Of the numerous caſes of hydatids related by writers, it may be remarked that hardly any of them proved fatal whenever they found an outlet. In the preſent caſe, the preſſure upon the neck of the bladder, and conſequent ſuppreſſion of urine, are evidently the cauſes of death. The manner of death from ſuch a cauſe is not unuſual; after a time, the pain produced by the diſtention of the bladder ceaſes, there are no more efforts to make water, and yet the patient dies ſuddenly. This ſudden death has been imputed to a tranſlation of the urine, as it has been expreſſed, to the brain. If any idea is to be annexed to theſe expreſſions, it muſt be, that the urine is effuſed either in the ventricles, or upon the ſurface of the brain; but no appearances of that kind were obſervable in the preſent caſe.

When the preſence of hydatids is ſuſpected, it would appear to be of great conſequence to procure them an outlet; but it muſt be obvious, that being generally ſeated in the abdomen, that buſineſs muſt be left intirely to nature. Upon the ſuppoſition that they belong to the animal kingdom, no internal medicine promiſes to be of ſo much uſe as mercury, which appears to have been given in one caſe wtih advantage *. But this is a

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ſubject there is no entering upon without a greater number of facts.

To this account I will ſubjoin a query reſpecting the manner in which the hydatids came to be lodged between the rectum and bladder. It has been obſerved that they are moſt commonly found in the liver and ſpleen, and in the preſent caſe their original ſeat would appear to have been in the laſt of theſe viſcera; may not, however, one of the ſacs or bags in the ſpleen have burſt, by which the contents would be ſpread all over the abdomen, and from their own gravity would naturally fall into the pelvis; and may they not have adhered to the neighbouring parts, and ſo multiplied there?

I have been more particular in relating the circumſtances of this caſe, becauſe ſuch ſeldom occur even to perſons in extenſive practice; in conſequence of which it often happens in rare caſes, that the views and experiments ſuggeſted by one caſe, are of no avail, for want of another opportunity to verify them. It will therefore be underſtood, that what is advanced reſpecting their being endowed with a life of their own, and alſo their mode of breeding or multiplying, is given merely as probabilities, and as well deſerving of further inveſtigation, whenever opportunities may offer.

Charles-Street,
April 17, 1787.

SUPPLEMENT.

In the year 1788, I had an opportunity of examining ſome hydatids that were found in the abdomen of a ſheep. Before I ſaw them the bowels had been taken out, and they were adhering to the fat about the kidnies, and alſo to the liver, and to the fat near the urinary bladder, in conſiderable number. They would appear to differ in ſome eſſential particulars from the human hydatids, yet they reſemble one another in ſo many things, that there is reaſon to infer therefrom, that both belong to the animal kingdom. It appeared to me, that a ſhort account of them would form a proper ſupplement to the caſe of human hydatids, read ſome time ago to the ſociety.

The hydatids in the ſheep were exactly the ſame with thoſe deſcribed by Tyſon *. They conſiſt of a mouth, neck, and oblong ſpherical body. The mouth had nothing of the cruciform appearance, if I may be allowed the expreſſion, that late writers have made the characteriſtic mark of *tænia*, and which they ſay is to be found in all hydatids. The mouth, examined with ſome care with the microſcope, appeared to be a ſimple longitudinal aperture. The neck was compoſed of rings, and there appeared very fine circles ſurrounding the body. They varied in ſize, from that of a cheſnut to the dimenſions of a turkey's egg. When put in warm water, though it muſt have been twelve or fourteen hours after the ſheep had been killed, they moved briſkly, with a kind of periſtaltic motion all over the body. Each hydatid was lodged in a ſeparate ſac, which was little more than ſufficient to hold it, for the neck was reflected upon the body. The ſides of the ſac were lubricated with a mucous fluid.

The human hydatids have no neck or mouth, and they are in great numbers in the ſame ſac, and of various ſizes. In theſe particulars they differ from thoſe of ſheep; nor could I find any young ones attached to the inſide of the hydatid, as in the human. In their mode of decay, however, they reſembled each other completely. The ſac became thicker and ſtronger, and at the ſame time diminiſhed in ſize, and compreſſed the coat of the hydatid into a ſubſtance like iſinglaſs. In the laſt ſtage this ſubſtance became like a mixture of chalk and water, and the ſide of the ſac hardened, and appeared in ſome degree petrified; for the hardneſs was more like that of a ſtone, than of a bone.

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* Phil. Tranſ. 1692.

I had, in the ſummer of 1791, an opportunity of examinnig the hydatids which are found in the brain of ſheep, producing the diſeaſe called, in ſome parts of the country, the *ſtaggers*. The hydatid is lodged in the ſubſtance of the brain; in one ſheep there were two hydatids, one in each hemiſphere of the brain; they were of an irregular oval ſhape; they had no mouth; their coats had the ſame appearance as in the hydatids found in the abdomen; and, when put in warm water, they had a ſtrong periſtaltic motion.

In ſome there were cluſters of young ones, adhering to their inner coats. Theſe were

ſome-

ſomewhat oval in their ſhape, and adhered by one end; but on detaching them carefully, and examining them with good magnifiers, I could never find the cruciform mouth deſcribed by ſome writers.

We may obſerve, that the mouth is not eſſential to the hydatid in ſheep, which renders it ſtill more probable, if not altogether certain, that the human hydatid is an animal *.

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* There being great reaſon to believe that hydatids form a diſtinct genus in the animal kingdom, and being ſo eſſentially different from the watery veſicles which are found in the *ovarium*, kidney, and *placenta*, it might be proper, in order to avoid ambiguity and confuſion, to diſtinguiſh them by a name peculiar to themſelves. Linnæus (a) calls the hydatid in ſheep *Hydra Hydatula*; but neither his generic character, nor ſpecific differences, which are chiefly taken from the mouth, apply to the animals in queſtion, in ſome of which there is no mouth. The Greek language, that inexhauſtible ſource of derivation, readily ſupplies us with a name. The word Ὑδρία, *Hydria*, is very appoſite. If that be adopted, we ſhall have *Hydria Humana*, *Hydria Ovilla*, &c.

(a) Syſtema Naturæ, Vermes, Zoophyta, Hydra.

V. *Caſe of a Gentleman labouring under the epidemic remittent Fever of Buſſorah, in the Year* 1780; *drawn up by himſelf*; *with an Account of various Circumſtances relating to that Diſeaſe. Communicated by* JOHN HUNTER, Eſq. F. R. S. *Read June* 17, 1788.

INTRODUCTION.

THE following caſe is that of a gentleman, not unacquainted with medical learning, tho' not of the profeſſion. To this circumſtance it is owing, that an explanation is neceſſary of the ſenſe annexed to the term *plague*, which occurs frequently. Our author uſes it in a ſomewhat vague ſenſe, but probably ſuch as is cuſtomary at Buſſorah; and calls by that name the *remittent fever*, which prevails every year, with various degrees of violence, in conſequence of the annual overflowings of the Euphrates. But there is no reſemblance between the plague, properly ſo called, and the remittent fever; they differ in their ſymptoms, progreſs, and cauſes, of which any one may eaſily ſatisfy himſelf, by conſulting the excellent treatiſe lately publiſhed by Dr.

Ruſſell on the Plague. There is further this remarkable difference between them, that they prevail in different ſeaſons of the year. The warm weather of the month of May puts an end to the plague at Buſſorah, and it is not to be ſeen in the ſubſequent hot months; whereas the remittent fever begins in the month of May, in which the Euphrates uſually overflows its banks in the neighbourhood of Buſſorah, and prevails chiefly in the warm months that follow. Our author frequently mentions the plague of 1773, ſuppoſing the remittent fever, and that epedemic, to be the ſame diſeaſe; but that is not the caſe. It was the real plague which raged at Buſſorah with ſuch violence in 1773; and from authentic accounts of it in the poſſeſſion of Dr. Ruſſel, it appears that it firſt ſhewed itſelf in February, increaſed in March and April, began to diminiſh early in May, and by the end of that month intirely ceaſed.

It is impoſſible to read the narrative of the ſufferings of the patient, without wiſhing that he had had the aſſiſtance of medical people, who had been acquainted with the proper mode of adminiſtering the bark in ſuch fevers. The numerous remiſſions he had, furniſhed the beſt opportunities of giving it;

and as in the end he owed his life to it, ſo there can be no doubt but a more early uſe of it, would have ſaved him from much ſuffering and miſery.

Ever ſince the great plague of 1773, in which it is eſtimated ſeven-eighths of the inhabitants periſhed, Buſſorah has ſeldom eſcaped, every third or fourth year, a viſitation from ſome malignant putrid diſeaſe. Whole families died, and remained unburied, until covered by the ruins of their houſes. Thouſands were interred in the ſtreets, but ſo near the ſurface, that I have ſeen the bones of the dead in many places appearing above ground. To complete the calamity, the inhabitants, in leſs than three years after this plague, ſuſtained a ſiege of thirteen months againſt the Perſians, in which they experienced every diſtreſs that the ſword, ſickneſs and famine, can inflict upon mankind.

The overflowing of the Euphrates, and its waters ſtagnating on the deſart, have always been accounted the leading primary cauſes of epidemical diſeaſes at Buſſorah. The great floods, from the melting of the ſnow on the mountains of Diarbekir, happened in the year 1780

1780 early in the month of May, when the heats in Perſia and Arabia began to be exceſſive. The deſart, which reaches to the gates of Buſſorah, is, for many miles, incruſted with a ſurface of ſalt, which, when mixed with the ſtagnated waters and expoſed to the ſun, produces the moſt noxious effluvia. So early as the 25th of May, the town was ſurrounded with a ſalt marſh, the heated ſteam ariſing from which was, at times, almoſt intolerable; but the canal that runs through a great part of the city being filled with the dead bodies of animals, and all kinds of putrid matter, and at low tides all theſe ſubſtances expoſed to the ſun, made the air in the town ſcarcely ſupportable; and being totally deſtitute of police, the ſtreets were in many places covered with human ordure, the bodies of dead dogs and cats, which emitted a ſtench more diſagreeable and putrid than any thing I ever experienced in my life.

From the accumulation of ſo many cauſes, it is not ſurpriſing that epidemical diſorders ſhould have reigned at Buſſorah in the year 1780; it is more wonderful that theſe diſeaſes are not both more frequent, and more fatal.

As to the degree of ſolar heat during the period of which I am about to ſpeak, it far

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exceeded what I conceived the human frame to be capable of bearing. The ſenſation under this heat was totally different from what I had ever experienced; it reſembled the approach of a heated ſubſtance to the body. According to Farenheit's thermometer the quickſilver roſe in the ſun to between 156 and 162 degrees. From the 30th of May I never ſaw it ſo low as 156, but generally between 158 and 160. The glaſs was fixed to a wall, covered at top, but expoſed to the reflected rays of the ſun. I am told, that after I left Buſſorah it was ſeveral degrees higher.

In the cooleſt part of the houſe, with the aid of every invention to decreaſe the heat, the quickſilver roſe to 115 degrees; but after I came away I was informed that it roſe ſtill higher, even at ſeven o'clock in the morning, the hour which we accounted the cooleſt in the day. Much about this time two gentlemen, who had hitherto eſcaped the infection, were taken ill, and fled from the factory; they reported that on the day on which they left Buſſorah, the heat was ſo intolerable that no one could expoſe himſelf to it long enough to obſerve the thermometer in the ſun.

I have heard from ſome of the oldeſt inhabitants of Buſſorah, that they never remembered to have felt, or to have heard of ſuch a degree of heat in any part of Arabia or Perſia. Before we were all taken ill, the natives of the country appeared more alarmed at the heat of the weather than the Europeans; nothing could induce them to expoſe themſelves to the ſun after ten o'clock.

From the 10th of May the people of Buſſorah began to foretell that the ſeaſon would be marked by a ſevere and general ſickneſs. This prediction was ſoon verified; towards the end of the month an intermitting bilious fever made its appearance in the town, which in a very few days ſpread with ſuch amazing rapidity, that upon a computation not a fourth part of the inhabitants were exempted from this calamity.

About the latter end of May I was ſenſible at times of extreme and unuſual internal heat and glowings, with ſenſations which I cannot deſcribe, and once or twice I perceived an inclination to ſhiver. I ſeldom made water more than once in twenty-four hours, and then in ſmall quantities, and of a very deep colour. My appetite and every other circumſtance continued as uſual.

I left Buſſorah for Aleppo the 30th of May. On our arrival at Zebire, the heat was ſo intenſe that even the Arabs ſunk under it. Moſt of the gentlemen who had accompanied me to this place felt it in a ſtill more ſevere manner. I was quite overcome; I felt an unuſual languor, weakneſs, alternate heat and cold, and many other ſymptoms which indicated an approaching fever. I could not immediately be prevailed upon to relinquiſh my journey; and although upon reflection I had not a ray of hope that I ſhould be able to proceed, yet at that time a diſappointment appeared to me equally dreadful with certain death in the deſart. At night every ſymptom increaſed, the enſuing day all hopes of purſuing my journey vaniſhed, and in the evening I was carried back to Buſſorah. In two days I got tolerably well, but pains in my back, loins, limbs, and great proſtration of ſtrength ſtill remained. About this time two gentlemen of the factory were ſeized with the fever, which now began to rage with violence.

5th June. From this day I date the actual commencement of my fever. About two o'clock after dinner I was ſuddenly attacked with a violent glowing heat all over my body, uneaſineſs, anxiety, and oppreſſion, but in a very inconſiderable degree to what I afterwards experienced; alſo a ſwelling in my tongue, which had been coming on ſome days, and is one of the firſt ſymptoms of the fever that prevailed. The fit continued about two hours; a ſlight perſpiration ſucceeded, which removed the fever, but left a head-ach, thirſt, and pains in my back and limbs. In the evening with aſſiſtance I got upon the terrace, when the moon and ſtars appeared of a bright yellow, and all objects had that colour through the whole of my diſeaſe; alſo the pain in making water and acroſs my loins became intolerable, like that felt in complaints of the ſtone in the bladder. I took ſome tartar emetic, which brought up a great deal of bile, and the next morning a purgative of Rochelle ſalts, manna, tamarinds, and anniſeeds.

6th June. In the forenoon a free, copious perſpiration, and a perfect intermiſſion of the fever; at night became very reſtleſs and uneaſy,

eaſy, could not ſleep, which I partly imputed to a draught of ſtrong muſtard whey, with ſome antimonial wine, which inſtead of cauſing perſpiration, produced the oppoſite effect.

7th. By the advice of a phyſician I took ſome weak decoction of bark, 1 oz. to two pints boiled to one, in the quantity of three tea-cups full before dinner. At three in the afternoon I had another hot fit, but not very ſevere. In the evening grew worſe, heat and thirſt exceſſive, drank muſtard whey on going to bed, but had a very bad night;—no ſleep, much oppreſſed, ſevere head-ach, and pain over my loins.

8th. I took a gentle purge of cream of tartar and manna, which operated and gave me ſome eaſe. Left off the bark, as it ſeemed to increaſe the febrile ſymptoms, and drank ſage and apple tea, decoction of prunes, tamarinds, &c. At ten o'clock in the forenoon a very ſevere hot fit; heat intenſe, oppreſſion in my ſtomach and breaſt almoſt inſufferable. Mr. —— ſurgeon of the Eagle cruiſer, gave me a moſt nauſeous ſaline mixture, which vomited and purged me ſeverely. The quantity of bile which came off my ſto-

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mach was incredible, yet I felt no relief, and the agony of the hot fit continued till four o'clock in the afternoon, when it went off by a moſt profuſe perſpiration. During this fit my thirſt was conſtant and intenſe. In the evening my ſkin became dry, the thirſt returned, and I had a very bad, ſleepleſs night.

I now began to experience ſome of the dreadful ſymptoms which are I believe peculiar to fevers in Turkey and Arabia, a ſenſation of dread and horror totally unconnected with the fear of death, for while the patient is moſt afflicted with this ſymptom, it is for the moſt part accompanied with a ſtrong deſire to put an end to his exiſtence. The agony from the heat of the body is beyond conception; I have heard ſome of my fellow-ſufferers roar hideouſly under the violence of the pain.

9th. Till noon tolerably well. About one o'clock the hot fit attacked me, and was full as ſevere as yeſterday; heat and thirſt rather greater, and but little relief for more than an hour after the perſpiration commenced. This attack left me very weak, much exhauſted with cold weakening ſweats, quick unequal pulſe, ſevere head-ach, confuſion, anxiety, and inceſſant thirſt; a ſleepleſs night, ſtartings, anxieties, and a conſtant wiſh to terminate my ſufferings by death.

10th. Forenoon, pretty free from fever. Attacked at the ſame hour as yeſterday. The fit more violent, delirium. The agony of the heat not to be expreſſed; the whole body as if on fire; unremitting thirſt, profuſe perſpiration, yet no relief till late in the evening; no ſleep, a dreadful night, &c. Pulſe about 120, unequal and fluttering.

A mere relation of facts can give but a faint idea of the wretched ſituation to which the factory was now reduced: by this time eleven twelfths of the inhabitants of Buſſorah were taken ill, numbers were daily dying, and the reports from Bagdad and Diarbekir of the increaſing ravages of the plague, left the ſurvivors not a ray of hope that they could eſcape from the calamity. On every countenance pain, ſickneſs and horror were ſtrongly painted; nor were we even left the comforts of ſympathy, as every mind was too much engroſſed with its own ſufferings to think of adminiſtering conſolation to others. Four of us lay under the portico of one of the ſquares of the factory, calling out for water in a phrenſy of thirſt. We uſed to ſnatch it from each other, and to ſupplicate for a mouthful with as much fervor as a dying criminal for an hour of further life.

About this period of the fever my eyes became very weak, and every object I ſaw was quite yellow. This effect was moſt perceptible at night, in looking at the moon and ſtars. In the evenings we were ſometimes carried in our cots upon the terrace of the factory for air, but the wind was ſo heated by the burning ſands of the deſart, that we felt it more intolerable than even the lower apartments. We all remarked that the Shemaal, or northern winds, which blew without intermiſſion at that time, greatly increaſed our heat and thirſt.

The daily very evident increaſe of my fever, and its effects upon others becoming every day more fatal and alarming, determined me, while any ſtrength remained, to embrace the conſul's offer of flying from the ſeat of infection to Buſhire, in the Ranger cruiſer.

11th. After an exceeding bad night I was carried early in the morning on board the Ranger, and was not very ill until about nine o'clock, when I felt the fever coming on, with new and more alarming ſymptoms, violent head-ach, giddineſs, dimneſs of ſight, approaching delirium, horror, and a moſt painful oppreſſion and burning heat in my ſtomach.

In deſpair, and to try to quench the unſufferable heat in my ſtomach and bowels, I took a pretty large doſe of nitre. The oppreſſion and pain increaſed, in my confuſion I took a paper of tartar emetic, which immediately began to operate. From that time, about ten o'clock, till half paſt two in the afternoon, I know but little of what paſſed. I was almoſt all that time either diſtracted with pain, or in a ſwoon; and had it not been for the extraordinary care and attention of the commander of the cruiſer, who ſupported me in his arms, and adminiſtered ſuch cordials as I, in the ſhort moments of recollection, could call for, I have not a doubt but I muſt have ſunk under this attack. He counted eight times that I fainted, and ſometimes an interval of ten minutes before he could perceive any ſymptoms of returning life. I was chiefly ſupported by wine, hartſhorn, and ſpirits of lavender. About three o'clock

I had recovered my recollection: moſt copious and continued ſweats had carried off the violence of the fever, but faintings and total privation of ſtrength and ſpirits remained upon me till late in the evening, when I became to all appearance, for a ſhort time, perfectly well. A little ſtrength returned, every ſymptom of fever vaniſhed, and my feelings were almoſt the ſame as if in perfect health. Some circumſtances having prevented the Ranger's carrying me to Buſhire, I was taken aſhore in the evening. When I was brought to the factory I had an appetite, and ate ſome chicken broth for ſupper. Mr. Roſs, who had hitherto eſcaped the fever, adminiſtered a potion of laudanum, and, I believe, antimonial wine, on going to reſt. I ſlept pretty well, and waked refreſhed in the morning. I, however, ſoon became ill; and at noon I had a ſevere attack, which continued three or four hours, and left me greatly weakened, my ſkin extremely dry, pulſe quick, fluttering, and irregular, beating from 100 to 120, with an unquenchable thirſt, which no liquids could allay. We had no acids of any kind, which we had great reaſon to regret.

I did not know till late in the evening that Mr. Abraham, the vice-conſul, who for ſome days paſt had been ill of the ſame fever, had determined to embark in the Eagle cruiſer next morning for Buſhire, as the only chance of ſaving his life; and a converſation which I overheard to this effect, that as I was ſo very ill, and no hopes of my recovery, it would be better to leave me behind to die at Buſſorah, made me ſtill more anxious to fly from the place, although I remember well I had not the moſt diſtant hope that I could live. I had ſuffered much at the factory, and in the peeviſhneſs of illneſs I thought (perhaps unjuſtly) that my living or dying ſeemed to be a matter of too little conſequence to thoſe, whom in health I had treated with much kindneſs and affection.

About ten o'clock, as I was lying in my cot, on a terrace adjoining to the ſtairs from whence the boat was to put off, I was ſeized with ſuch a fluttering, palpitation, ſtarting, difficulty of utterance from the ſwelling of my tongue, that I lay in momentary expectation of breathing my laſt. This was, however, probably the cauſe of my hearing the preparations for the departure of the boat.

About midnight they were leaving the ſhore. I could not make myſelf heard, and I was too weak to get up without help. I made ſeveral efforts, and at laſt overſet the cott I was lying upon, and brought myſelf to the floor, from whence I crawled on my hands and knees to the ſide of the river. Humanity pleaded for me, and I was taken into the boat in a ſituation of wretchedneſs I never can forget. We were, after being ſeveral hours on the Euphrates, carried on board the Eagle, oppoſite to Margil, a country houſe belonging to the factory, a few miles diſtant from Buſſorah.

12th. The day was uncommonly hot, and my fever came upon me about ten o'clock. The heat was intenſe. Mr. ——, a young unexperienced Frenchman, gave me tamarind water and cream of tartar, which had not a good effect. I now diſcerned the firſt ſymptom of a cold fit, but it was ſlight, and of ſhort duration. This day, however, I ſupported the fever rather better than uſual, and in the evening had a ſhort intermiſſion, and ſlept a little during the night. The air on the river was this day cooler and more refreſhing than on ſhore.

13th. About eleven o'clock had a regular cold ſhivering fit, ſucceeded by a very ſevere hot fever, which continued till five in the evening, when I was ſomewhat relieved by perſpiration.—This day the agony of the hot fit was inexpreſſible, with great pain in my loins, and a conſtant inclination to make water, which came from me in drops like blood. —I had a very bad ſleepleſs night.

14th. By Mr. ——'s advice, I took a doſe of tartar emetic, which not working, he gave me ſome ipecacuanha. I brought up a great deal of bile, but the fever increaſing, my ſufferings under it were greatly increaſed, by the operation of the emetic, which worked powerfully both upwards and downwards.—This was a trying day indeed. I can give no idea of what I ſuffered, which muſt have been intolerable, attended with intervals of delirium, and frequently ſwooning. About five the fever began to abate a little, and at ſix Mr. —— gave me a ſmall doſe of decoction of bark, which ſeemed inſtantly to cauſe a return of the fever, heat, thirſt, anxiety and pain.

My fellow-ſufferer, Mr. Abraham, was in violent agony this day; he cried out repeatedly, that a fire was conſuming his bowels, and that he was in exquiſite torture. The captain of the cruizer had been complaining; he, Mr. Abraham, and myſelf, lay in the ſame cabin. About four o'clock in the afternoon, when my pain was exceſſive, I crawled from my cott, with an intention to drop myſelf from a port-hole which was under the captain's bed, into the river. I had nearly accompliſhed my purpoſe, when the captain perceived me, and had me carried into my bed. I was not delirious; and, in ſpite of all my reſolution, the agony I ſuffered this day made me repeatedly ſcream out. One of our ſeamen died ſuddenly, and the blood, I was told, inſtantly flowed from all parts of his body.

This evening we arrived at Buſhire. On our coming to an anchor, Mr. Abraham was immediately carried aſhore; but I was too ill to be moved, and accordingly followed in the morning with Captain Sheriff, after a very ſevere ſleepleſs night.

The heat was ſo exceſſive this day, that even the natives we had on board ſunk under it, and many of them were ſtruck down with the ſun. By noon nobody could keep the deck; and about this time the veſſel run aground on the bar at the mouth of the Euphrates, but fortunately beat over, and got into the gulph. Two more of our people died in the evening; and I ſhould have mentioned, that, in ſailing down the river, we ſaw them throwing many dead bodies from the veſſels which were at anchor below Buſſorah, and many boats crouded with people from the Arabian ſhore, paſſing over to Perſia.

The Perſians at firſt oppoſed the landing of our ſick, and threatened to burn the ſhip; but they were prevailed upon, by the company's agent at Buſhire, a moſt worthy man, to permit us to come aſhore.

15th. I was ſo weak, that it was with difficulty they could carry me aſhore in my cott; my ſtrength was quite gone, and I was helpleſs as an infant. Some grapes, water-melon, and ice, had been got for Mr. Abraham: of the latter he had eat freely; I was much preſſed to do the ſame, but I was afraid of its increaſing my pain, and could hardly be prevailed upon to taſte it; I, however, ſwallowed a little, but by this time my tongue and throat were ſo ſwelled, that I had difficulty

culty in getting any thing down. About ten o'clock I was attacked as uſual, but I was become ſo weak, and the fever running even higher than uſual, that I appeared in the courſe of this day, more than once, to be in the agonies of death; perſpiration gave no relief to the violent pain and oppreſſion I laboured under.

The factory at Buſhire is a miſerable, wretched, mud building, bearing much more reſemblance to a ſtable than a human abode; the few rooms, or rather cells, are inſufferably hot, even to thoſe in health, and the reſt of the building has no cover from the ſun. In one of the beſt of theſe receſſes in the wall Mr. Abraham and I were placed, and on the ſame bed, as there was not room for two; our agonies were great, our cries dreadful. About ſeven o'clock in the evening I perceived my companion in the agonies of death. The company's agent, Mr. Beaumont, moſt humanely afforded him every aſſiſtance in his power; and, when every other perſon was afraid to come near us, he himſelf attended, and adminiſtered ſuch cordials as he thought might give us relief. Mr. Abraham died in great pain; and for fear of alarming the inhabitants, or rather commandant or ſheik of the town, Mr. Beaumont thought it neceſſary to conceal his death. It was ſome time before the dead body could be removed, which had become very putrid, and covered with purple ſpots. I have been ſince told, that immediately after death great quantity of blood or bile flowed from him, as black as ink, and ſo highly offenſive, as to be ſmelt at ſome diſtance from the factory. Paſt midnight we were both removed to the terrace, but, unfortunately for me, there was only one ſpot where we could lie, and the ſmell of the dead body was become intolerable; I was, however, by the humane aſſiſtance of Mr. Beaumont, cleaned and put into another bed, who ſat by me, and treated me with uncommon tenderneſs. I recovered a little, but paſſed a dreadful night indeed, I ſhuddered at the agony which I was to feel on the return of the ſun, and moſt anxiouſly wiſhed for death, as the only relief from pain, that I could expect. I was unable at this time to move hand or foot, and at times could not ſpeak. I told Mr. Beaumont that I thought a gentle doſe of phyſic, if it could be got down, might alleviate the racking pain in my breaſt, ſtomach, bowels, and loins; he accordingly prepared ſome ſalts, manna and tamarinds, and gave it to me in the morning.

16th. At eleven o'clock the violence of the fever came on; I grew delirious, ſwooned, and the ſymptoms of approaching death, I was afterwards told, grew evident to thoſe around me. My eyes were fixed, my tongue hung from my mouth, and my face grew quite black. I recovered from this fit about twelve o'clock, and felt excruciating pain, and a burning ſuffocating heat. My ſtomach and bowels ſeemed all on fire, my lungs played with the utmoſt difficulty, and I felt a pain and ſenſation about my heart which I cannot deſcribe. I was unable to move; my ſervant lifted me; I fell into a ſwoon for a few minutes, and, when I came to myſelf, a great quantity of black putrid bile flowed from me. Relief was inſtantaneous, and I ſlept or ſwooned till about five o'clock, when I found myſelf free from fever, and able to ſpeak, my recollection clear, and my mind perfectly compoſed, but my body ſo weak that I had no power of moving, except one of my hands. They gave me ſome ſuſtenance; I had a little ſleep; but about midnight I fell into a ſituation, which I had all the reaſon to think indicated the immediate approach of death. My tongue cleft to my mouth, my extremities were as cold as ice, and the coldneſs alſo appeared to extend up my thigh; my arm was deſtitute of pulſe, nor was the ſmalleſt pulſation of the heart perceptible; I never had my recollection clearer, or perhaps ſo clear, in my life. My ſervant was lying by my bedſide; I was convulſed for ſome minutes; and, on recovering, I got out the word Boy.—Fortunately for me he was not aſleep, and heard me; I then got out the word Wine; on which he brought me a glaſs of claret, which, with much difficulty, I got down; I felt myſelf much revived; I reflected on my ſituation; and, although I had not the moſt remote idea of ſurviving that night, I recollected that I had ſome fine powdered bark in my trunk, and it occurred to me, that if any thing could be done to preſerve my life, it would be that medicine taken in red wine; but my ſpeech immediately failing me, I could not direct the ſervant to give it to me. Death ſeemed approaching; coldneſs had ſeized all my limbs; my ſight became confuſed, as I perceived from looking at the ſtars, which danced before me; and the rattle or noiſe in my throat was very perceptible to the ſervant, as he afterwards told me. I fainted, and continued in a ſtate of inſenſibility, I believe, for about an hour. The loud lamentations of the ſervant, bewailing his own misfortune in loſing his maſter in a country ſo remote from his own, ſeemed to recal me to life. I felt as if refreſhed with a little ſleep, and got out the words *bark and wine*; it was inſtantly brought, and the man gave me two large tea-ſpoonfuls, in a large glaſs of claret. The effect was inſtantaneous, and operated like a charm; the coldneſs left me, I could ſpeak intelligibly, and could move my hands. I told the ſervant to give me a tea ſpoonful of the bark every hour, in a glaſs of claret. By eight in the morning I had taken ſix doſes, and more than half a bottle of claret. I was conſiderably ſtrengthened, and could converſe with Mr. Beaumont, who encouraged me to perſervere in the bark, and treated me with uncommon attention. I had been ſadly neglected at Buſſorah, but this was amply made up to me by the humane and tender attentions of Mr. Beaumont, who was a great predeſtinarian,

deſtinarian, and who never ſhunned danger when he felt it a duty to aſſiſt a fellow-creaature. He waited upon me like a nurſe, conſoled me under pain and ſickneſs, and, when my fever was at its greateſt height, he has often held me in his arms, when I wanted to be removed, or my bed ſhifted. About this time my legs and thighs became covered with blotches of a duſky brown hue, ſome of them as broad as the palm of the hand, quite dry, and they itched intolerably. At the ſame time ſeveral little boils broke out in different parts of my body, but there was only one, over my eye, which came to ſuppuration; the others, and the eruption on my legs and thighs, all diſappeared.

I continued the bark till twelve o'clock, and then left it off till four, when I took another doſe. The dreadful fever of the preceding days did not return on this, but I was ſtill extremely ill, had very great difficulty in ſpeaking and breathing; a ſwelling alſo in my throat, parched tongue, and unquenchable thirſt. I had not the moſt diſtant hope of living. I tried to take ſome broth, but the ſwelling in my throat prevented my ſwallowing. I paſſed a very bad night, with ſtartings, anxiety, and great pain over the kidnies, and what little ſleep I got ſeemed to make me worſe; I was fatigued with it, and under the conſtant dread of ſuffocation; towards morning my throat grew worſe, and my thirſt was exceſſive.

18th. Left off the bark, uncertain what I ought to do—no fever, but the ſame ſymptoms as the day before—drank a little chicken-broth, which was the only ſuſtenance I had taken for four days before; great oppreſſion and heat in my ſtomach and bowels. Mr. Beaumont found out an Armenian, who profeſſed phyſic. This man gave me a clyſter, which gave me great relief, and a water to drink, famous as a febrifuge among the Perſians; I drank of it freely, and found much benefit from it. But the moſt extraordinary of all the ſymptoms I experienced was this, that the third day after the firſt intermiſſion of my fever, one of my teeth, and one of the nails of my hand, came out without the ſmalleſt pain, only a little ſwelling in the gum, and, on the nail falling off, ſome matter flowed from the end of my finger; I never had had the tooth-ach; at this time the boil on my eye ſuppurated.

From the 18th of June to the 5th of July, being ſeventeen days, my fever did not return. I recovered ſtrength ſlowly, and could walk a little, ſupported by two men. My food was chiefly chicken and veal broth, and about a glaſs and a half of Madeira wine a day. Yet I had ſtill many ſymptoms of diſeaſe hanging about me — reſtleſs fatiguing nights—great thirſt—bad taſte in my mouth, every thing I took ſeemed bitter and ſalt; pains in my back, ſides, and loins, and great difficulty in making water. In this time I paſſed much bile, naturally and by clyſter, and I had a purgative from the Armenian, which weakened me greatly.

The ſprings lifted, as ſeamen expreſs it, the 1ſt of July, that is three days before the change. The opinion is univerſal in thoſe countries, and alſo in India, particularly Bombay, where intermittents are prevalent, that the change and full of the moon has an effect upon all intermitting diſeaſes, of which afterwards I had many proofs in my own caſe. To prevent a relapſe, I took ſome decoction of bark, but in too ſmall quantities to anſwer the purpoſe.

4th July. My water, from being thick and muddy, became quite clear.

The 5th of July, after dinner, I was taken with a ſlight hot fit, which continued about two hours, and then went off by copious perſpiration; the remaining part of the evening I was intirely free from fever.

4.

6th. I took decoction of bark—no fever.

7th. At eleven o'clock a regular cold and hot fit; the former continued three quarters of an hour, the latter two hours. Although much leſs ſevere than my former fever, yet I ſuffered a good deal from the hot fit. Left off the bark by the advice of the Armenian, who told me that it heated me, and made me worſe.—Much weakened by this day's illneſs—nor did the perſpiration intirely free me from the fever.

8th. Had a clyſter thrown up, and early in the morning took a draught from the Armenian, compoſed of fine vinegar, ſugar, and a country ſeed infuſed overnight in water.—A ſlight hot fit, but of ſhort continuance.

9th. The draught of yeſterday repeated—at ten a regular cold and hot fit, rather leſs violent than the laſt.

10th. No ſever—tolerably well.

11th. Between eight and nine in the morning a cold and hot fit, much more ſevere than the laſt—great heat, thirſt, and oppreſſion—much weakened. I now perceived I had got a regular tertian, and determined on the bark; but was prevailed on by the Armenian not to uſe it, he promiſing to cure me in a day or two. He gave me water-melon, and his infuſion of vinegar and ſeeds.

12th. No fever—find the water-melon to diſagree greatly with my ſtomach.

13th. Had not as yet indulged much hopes of a recovery. I felt ſtill, even in thoſe days previous to this laſt relapſe, many alarming ſymptoms of diſeaſe, which made me apprehend I could not recover; and this laſt attack had again reduced me ſo low, that it was evident that, unleſs I could get removed from thoſe ſcorching climates, a very ſhort time would put an end to my life. Except in the humanity and goodneſs of Mr. Beaumont, I was without a ſingle comfort or conveniency of life at Buſhire. The heat of the weather ſeemed daily to increaſe, and the houſe we were in hardly covered us from the direct rays of the ſun. My ſervant was taken ill, and appeared to be dying: it was with the greateſt pleaſure, therefore, I received the accounts this day of the Eagle cruizer having arrived laſt night from Buſſorah, on her way to Bombay.

At half paſt ſeven o'clock I had a very ſevere fit—the hot fit was uncommonly violent, and continued about three hours. I was much reduced, and reſolved, at all events, to take the bark in powder and in large doſes, and diſmiſs the Armenian. In this I was confirmed by Mr. Puget, who informed me that the few ſurvivors at Buſſorah owed their lives intirely to the bark, which had at laſt been given in very large doſes.

14th. I took four doſes, or eight teaſpoonfuls, of powdered bark. It purged me, and carried off a great quantity of black putrid bile.—This evening I was carried in my cott on board the Eagle; reſolved at all events, even if I had been certain of dying in the boat, to leave Buſhire, where I had hardly ſhelter from the ſun, and where the heat was ſo exceſſive, that Capt. Alderſon of the Eagle, and two paſſengers, were taken ill from it laſt night. I got on board very late, yet found myſelf much refreſhed from the ſea air.

15th. The fever did not return.—I continued to take the bark as yeſterday, and found myſelf ſurpriſingly ſtrengthened. I perſevered under this courſe till the 3d of Auguſt, when the veſſel arrived at Muſcat, and I was aſtoniſhingly recovered for the ſhortneſs of the time.

From the 14th of July to the 3d of Auguſt I had taken ſeven ounces of bark; and as the fever had returned upon me the laſt day of the ſprings at the preceding change of the moon, two days before this change I increaſed my daily doſe, and continued in this manner till the 4th, when the ſprings being over, and perceiving no ſymptoms of fever remaining, I left off the bark intirely. I had generally taken ſix tea-ſpoonfuls every day.

From the 15th of July till my arrival at Muſcat, a ſea-port on the coaſt of Arabia, my recovery was exceedingly rapid. I had a keen appetite, a pretty good digeſtion, ſound refreſhing ſleep, and my daily increaſe of ſtrength was very perceptible. My diet, till this time, had been generally chicken broth, rice, and boiled fowl, light pudding, &c. On leaving off the bark I obſerved no particular regimen, only abſtaining from ſalted and high-ſeaſoned meats, and confining myſelf to three or four glaſſes of Madeira. I found that an infuſion of prunes, with a ſmall quantity of cream of tartar, was of much ſervice to me during the courſe of the bark, as it kept me cool, and my body open. I was ſenſible, at times, during my recovery, of a ſlight but troubleſome pain under my fifth rib on the right ſide, eſpecially when I lay on that ſide; but from feeling and preſſing my hand over the region of the liver, and from other circumſtances, I had no reaſon to ſuſpect that my liver was affected; and as it ſoon left me, the cauſe was probably trifling or accidental.

On my leaving Muſcat, a large boil came upon the hip-bone, the ſize of a ſmall melon, extending ſome way up the ſide and down the thigh, with a hard baſis. After arriving at Bombay, which we did in fourteen days, it broke, and in a few days healed up. I ſhall only add, that at Bombay I was detained four months, before I had an opportunity of proceeding to Europe. In that time I had three returns of my ague, but, on taking a few doſes of bark, it left me. Thoſe attacks happened at the change of the moon. From Bombay to Europe I had three or four ſlight fits of the ague; the worſt on our making the coaſt of South Guinea, at the ſettlement of Benguela, where we found the wretched remains of a Portugueze garriſon, the ſurvivors of a fatal putrid fever, which, as they told us, raged in thoſe parts for eighteen months before. The laſt attack I had was the day we made the Rock of Liſbon, ſince which time I have had no returns of the ague, although, when the wind continues long at eaſt, I am ſenſible of a tendency to that complaint.

I ſhall now give a brief account of the fate of my fellow-ſufferers at Buſſorah. This unfortunate party conſiſted of Capt. Sheriff, of the Eagle cruiſer; Mr. Brown, a Bengal merchant, carrying goods from India to Aleppo; Mr. Palmer, a gentleman returning with his fortune from Bengal to Europe; Mr. Robſon, ſurgeon to the factory; Mr. Abraham, the vice-conſul; Dr. Roſs, who had practiſed many years at Conſtantinople, ſome time in Bengal, and was then taking the opportunity of accompanying me acroſs the deſart; Mr. Smith, a merchant from India; and an Italian Carmelite, the vicar of Buſſorah, who came from Bombay. It is unneceſſary to ſay, that the *ſeeā dāb* is a common ſymptom in the Turkiſh fever, or, in other words, a ſtrong deſire of ſelf-deſtruction. We had a fatal inſtance of it in our party. Mr. Brown, the ſecond day of his fever, being left alone, got to his piſtols, and, throwing in four or five balls, diſcharged it into his breaſt, and was found dead a few minutes afterwards. I believe every one of us at times would have done the ſame, had we been poſſeſſed of the means of accompliſhing it. Mr. Robſon died the third day of his fever in great agonies, but perfectly ſenſible; his was a continued high fever, without any remiſſion. Mr. Palmer died the fourth day under the ſame ſymptoms as the preceding. The Carmelite, the ſecond morning after he was taken ill, had opened a vein in his arm, and bled to death, moſt probably intentionally. Captain Sheriff was ſeized with the fever on his return from Buſſorah to Buſhire. He died on the third day in a manner which is even painful to relate. He was a man of ſingular ſtrength of conſtitution,

tution, and ſuffered unuſual agonies before he died. His cries were heard all over the factory; he foamed at the mouth, gnaſhed his teeth, and tore his arms with his teeth. Thoſe who heard him compared his cries to the bellowing of a mad bull. He was no ſooner dead but his body was covered with purple ſpots, and ſo offenſive that the people could hardly carry it out to be buried. Mr. Sheriff's was what they call the worſt kind of plague.

Mr. ——, whom I have mentioned, was an uncommon character. He was ſhut up in a mud houſe at Buſſorah during the great plague in 1773, which he eſcaped. He had fortunately a large ſtock of Bengal cotton goods with him, which he diſpoſed of to the inhabitants to wrap their dead in. They uſed to depoſit the price in a baſket, which by means of a long rope he let down, which was then hauled up, and lowered again with the cloth purchaſed. He had an account of 70,000 winding-ſheets which he ſold during the calamity. This fact was atteſted by Mr. Abraham and the conſul. He was a well informed ſenſible man, and an excellent linguiſt. On his voyage to Buſſorah he was ſeized with a ſpecies of inſanity: he ſuſpected the captain and ſupercargo of his ſhip to have conſpired his death, and he fancied he overheard their deliberations on the ſubject: he always ſlept with piſtols in his cott, and refuſed all manner of communication with his conſpirators, as he called them. He then imagined himſelf poſſeſſed of the ſingular faculty of overhearing, by the peculiar conſtruction of his ears, the moſt diſtant whiſper to his prejudice. Although ſuch an idea was abſurd, yet he could argue upon it with a great deal of ingenuity. In all other reſpects he was perfectly reaſonable, and converſed on every other ſubject with much good ſenſe and propriety. On the breaking out of this ſickneſs at Buſſorah he ſhut himſelf up in the upper room of an Armenian's houſe, and had no intercourſe whatever with the other Europeans. It was not known how he exiſted; he, however, eſcaped the infection. He afterwards came to England, where I have ſeen him, and he appeared perfectly well.

Dr. Roſs, whom I have mentioned, on the firſt rumour of the plague put himſelf under a courſe of bark, which he continued taking in large quantities while I remained at Buſſorah.

rah. I have ſince ſeen him in England, and he told me that when the illneſs began to abate, he was taken with an intermitting fever; that he fled from Buſſorah in an open boat towards Bagdat, but was taken by a party of Arabs, who carried him upon the deſart. He contrived to get away from them, croſſed the deſart to Aleppo, and afterwards got to England.

It was computed that 275,000 died in the town of Buſſorah of the plague of 1773, and that 25,000 died in the town and neighbourhood of this laſt plague. We heard of its approach from Aſia Minor; it ravaged Diarbekir, antient Aſſyria, and kept the courſe of the river Tygris to Bagdat, where many died. From thence it followed the courſe of the Euphrates to Buſſorah, and for about twenty miles lower. The oppoſite, or Perſian ſhore, although within a few miles, was exempted; nor did it ſpread more than a few miles into the deſart.

The firſt ſymptoms of this fever, or plague, are generally ſwelling of the tongue, a violent head-ach, bleeding at the noſe, pains all over the body, a conſtant inclination to make water, which comes in drops, and attended with great pain, and is as high-coloured as blood; (if the urine, on ſtanding, becomes purple, it is ſaid to be a certain ſign of death) extreme heat, great apprehenſion, all objects appearing of a yellow colour, uncommon terror, and at the ſame time a great deſire for death; there are alſo boils or eruptions on the ſkin, which moſt commonly appear either juſt before the diſeaſe proves fatal, or the patient begins to recover.

VI. *On the Want of a Pericardium in the Human Body. By* MATTHEW BAILLIE, M.D. F.R.S. *and Phyſician to St. George's Hoſpital. Read Dec.* 16, 1788.

IT is of importance to aſcertain any deviation from the ordinary ſtructure of animals, becauſe it either aſſiſts in explaining ſome circumſtance in the animal œconomy, or tends to give a more accurate view of nature. This, however, is not equally the caſe with every deviation; but it is of more conſequence to aſcertain it in proportion as it may be rare, as doubts may have ariſen with regard to it, or as it may be more connected with any great operation of an animal body. In which ever of theſe lights we conſider the ſingular phænomenon of a want of pericardium, it is very worthy of being noted.

This is one of the deviations from the ordinary ſtructure of an animal in which nature has been moſt ſparing; very few anatomiſts have had an opportunity of obſerving it, and the deſcription which thoſe few have given has been very imperfect*. The appearance alſo of an adheſion of the pericardium to the heart might be miſtaken for a want of this membrane altogether; and hence the beſt modern anatomiſts have conſidered this luſus as never having taken place. Even Haller, who was by no means diſpoſed to be incredulous, has denied its exiſtence. An opinion thus taking its origin from the authority of the moſt eminent anatomiſts, had at length ſpread into a general belief that the want of a pericardium had never occurred in the human body. It will appear, however, that this general opinion has been taken up too haſtily, and that ſuch a luſus does ſometimes happen.

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* I have met with the following inſtances of a want of pericardium being obſerved by different anatomiſts.

"Diſcipulum item in Romanâ Academiâ mortuum ſecui (aderat excellens Medicus Alex. Trajanus Petronius, a civitate Caſtellanâ, acris judicii vir) huic miſero juveni pericardium deerat, itaque ſubinde in ſyncopen incidebat, ſubinde mortuo ſimilis conſpiciebatur, quo genere morbi exanimatus eſt."—Reald. Columb. Lib. xv. p. 265.

"Cor pericardio plane denudatum quod & ſemel Columbo obſervatum. Dextra auricula circa venæ arterioſæ orificium aneuryſmatis modo ultra juglandis magnitudinem diſtenta erat." Vid. Barthol. Centur. iv. Hiſtor. 20. p. 266.

"Monſ. Littre a trouvé dans une femme de 54 ans le cœur ſans pericarde & enferme abſolument à nud dans la cavité de la poitrine. Il etoit ſec dur d'une ſurface inegale, raboteuſe, il avoit peu de graiſſe & une graiſſe peu onctueuſe. On voit aſſez par ce qu'il y avoit d'extraordinaire dans ce cœur a quoi doit ſervir le pericarde, &c."—Memoires de l'Academé Royale, p. 37. 1712.

Upon opening into the cavity of the cheſt in a man about forty years of age, in order to explain at lecture the ſituation of the thoracic viſcera, I was exceedingly ſurprized to ſee the naked heart lying on the left ſide of the cheſt, and could ſcarcely at firſt be-

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" Il naquit à Grenoble un fœtus monſtrueux mort mais qui ſa mere avoit ſenti remuer peu de tems avant ſa naiſſance," &c.

" Celui la (ſcilicet le Fœtus) portoit ſon cœur en dehors pendu a ſon col comme une medaille de ſorte qu'il pouvoit aller & venir ſur la poitrine. Ce cœur etoit d'une conformation naturelle ſans pericarde, attaché a ſes gros vaiſſeaux qui lui tenoient lieu de cordons & qui etoient a decouvert comme lui. M. de Vaubonnais envoya cette relation à M. Parent bien atteſtéé par des medicins & des chirurgiens de Grenoble."—Id. p. 39.

" Qu'il (ſcilicet le Cœur) puiſſe etre ſans pericarde je l'ai vu dans un chien fort vigoureux, mais je ſuis ici un temoin recuſable, mes amis ſçavent pourtant que j'ay de la bonne fai par excez & que je ſuis ſincere dans le choſes meſme ou mon interet m'obligeroit de diſſimuler."—Diſcours iv. p. 111, 112. Paris 1685.

" In fine, I have obſerved in a new-born female infant the heart without a pericardium, and turned upſide down, ſo that its baſis, with all its veſſels, had fallen down as low as the navel, and its apex, ſtill on the left ſide, lay hid between the two lungs." Philoſoph. Tranſactions, An. 1740—1741. N° 461.

lieve what I ſaw, but the circumſtances were too ſtriking to keep me long in doubt. The heart was as bare and diſtinct as it commonly appears in opening into the cavity of the pericardium, and every collateral circumſtance confirmed the fact.

The mediaſtinum conſiſted, as in common caſes, of two laminæ of pleura; but it was ſomewhat changed in its direction, being inclined to the right ſide of the cheſt, and lying upon the right of the heart. Both laminæ were connected together through the extent of the mediaſtinum by the common intervention of the cellular membrane, and croſſed over the vena cava ſuperior about an inch above its entrance into the auricle. The heart lay looſe in the left cavity of the cheſt, unconnected in any way except by its veſſels; was of a large ſize, elongated in its ſhape, and had its apex oppoſite to the eighth rib. The right auricle was obviouſly in view in the ſame manner as when the pericardium has been opened, and the vena cava ſuperior and inferior were clearly obſerved entering into it. The appendage of the left auricle was as clearly in view; and when the heart was inverted, ſo as to have its apex turned upwards, the extent of its cavity was ſeen, with the two pulmonary veins of the left ſide entering behind the appendage. The right and left ventricles were diſtinct, with the coronary veſſels running upon them; and the aorta and pulmonary artery were ſeen clearly emerging from them.

The heart was involved in the reflection of the pleura belonging to the left ſide of the cheſt, which became its immediate covering, and, upon making the ſlighteſt inciſion into the ſubſtance of the heart, its muſcular ſtructure was laid bare, as in any common heart deprived of its pericardium. There was no connection between the heart and the diaphragm, but they were entirely ſeparate, and the diaphragm oppoſite to the flattened part of the heart was covered only by a reflection of the pleura. It is well known, that, in ordinary caſes, a portion of the pericardium adheres firmly to the diaphragm, which forms a medium of connection between it and the heart. In adheſions, too, of the pericardium to the heart, its attachment to the diaphragm is always the ſame as in the ordinary healthy ſtructure of theſe parts.

The apex of the heart being lower down than uſual, there was a deficiency of the left lung correſponding to this change of ſituation.

The phrenic nerve of the right ſide ran between the two laminæ of the mediaſtinum, near that edge of it which was applied to the right ſide of the heart. The left phrenic nerve ran between the ſame two laminæ of the mediaſtinum, almoſt immediately under the ſternum. This is a great deviation from its natural courſe, for it commonly paſſes on the outſide of the pericardium, following the obliquity of the left edge of the heart.

All theſe circumſtances were ſeen upon ſimply removing the ſternum with a ſmall portion of the ribs, and therefore put the want of pericardium in this ſubject beyond all doubt.

In the adheſion of the pericardium to the heart, the direction of the mediaſtinum cannot be altered, the heart cannot be ſeen lying looſely in the cavity of the cheſt, unconnected with the diaphragm, the appearance of its ſeveral cavities and veſſels cannot be diſtinct, nor can any of the other circumſtances take place which we have mentioned.

Did the aſcertaining of this ſingular luſus naturæ require any other evidence than the deſcription which we have given, it would happen to be ſupported by a very large teſtimony, for it has been ſeen by many medical gentlemen in this metropolis, who are eminently ſkilled in anatomy, and by a great number of ſtudents, who are very capable of judging.

In this luſus we may remark, that the mediaſtinum paſſed ſomewhat to the right, ſo that the heart was intirely in the left cavity of the cheſt. Upon a little reflection we ſhould ſuppoſe that this would happen. Had the mediaſtinum been joined to the body of the heart, it would have been diſturbed in its function, from being confined by a tight cord to the ſternum. To avoid this inconvenience, the mediaſtinum was ſituated a little more to the right, and the heart a little more to the left, than common.

The right phrenic nerve paſſed very naturally between the laminæ of the mediaſtinum, for this was nearly its uſual courſe; but that of the left phrenic nerve was neceſſarily much changed. It could not paſs over the body of the heart, or of the lungs, which were each of them to be in conſtant motion; it had therefore to ſeek for ſome fixed channel, and the only one was in the mediaſtinum. Accordingly it was found, as deſcribed, between the two laminæ of the mediaſtinum, almoſt immediately behind the ſternum.

It was natural to ſuppoſe, as there is a deficiency of the lungs in ordinary caſes, oppoſite to the apex of the heart, that where the apex of the heart was altered in its ſituation, there ſhould be a change in the ſituation of the deficiency in the lungs correſponding to it.

It may be a queſtion, how far the increaſed bulk and elongated ſhape of the heart, in this particular inſtance, depended on a want of the pericardium? It might be ſuppoſed that the heart, being free from its uſual reſtraint, had grown beyond its common limit. This, I think, however, will appear, after a little reflection, not to have been the caſe. The heart, under the common circumſtances, is originally incloſed in a pericardium which grows along with it, and the increaſe of growth keeps pace in each. If, then, there ſhould be at any time a diſpoſition to form a large heart, there is alſo formed a large pericardium to contain it; or if the heart be ſmall, there is proportionably a ſmall pericardium. If it be ſtill ſaid, that the pericardium tends to limit the growth of the heart, a queſtion naturally ariſes, What checks the growth of the pericardium? Is it a principle within itſelf? Why ſhould not the heart be poſſeſſed of the ſame principle? I conſider, therefore, the increaſed bulk and elongated ſhape of the heart, in this particular inſtance, as an accidental circumſtance, and by no means depending on a want of the pericardium.

From the want of a pericardium in an adult who had arrived at the middle period of life, it is reaſonable to think that its uſe is not very eſſential. One uſe which has been attributed to the pericardium is, the ſecretion of a liquor to lubricate the ſurface of the heart for its eaſy motion. This ſort of reaſoning would appear *à priori* to be ill founded. It is is impoſſible to ſee how a bag like the pericardium, or any other bag, ſhould be peculiarly fitted for this purpoſe. The ſurface of the heart itſelf, and of the lungs, might have had a power of ſecretion; and accordingly we find in this particular ſubject a fluid ſupplied by them for lubricating the heart without a pericardium. This account of the uſe of the pericardium was very unſatisfactory to the mind of Haller, and upon the grounds we have mentioned.

The more ſtriking uſe of a pericardium is to keep the heart in a ſteady ſituation, ſo as to enable it to carry on its function uniformly. This one is more diſpoſed to conſider as its uſe from its being a very ſtrong membrane, capable of bearing a good deal of reſiſtance; and from its being firmly attached to the tendinous portion of the diaphragm, which preſerves always nearly the ſame ſituation. If we reflect, however, a little, we ſhall perceive, that a heart without a pericardium is not really left looſe in the cavity of the cheſt, but is confined in a great meaſure by the parts being exactly adapted to each other, and completely filling up that cavity. The heart may in this manner be ſufficiently confined to perform its function, although it cannot be ſo ſteadily maintained in the ſame ſituation as when encloſed in its pericardium.

Such are the circumſtances which preſent themſelves to the mind as matter of reaſoning in the caſe before us; but I was anxious to learn ſomething of the hiſtory of the perſon during life, becauſe it appeared not unlikely that it might throw light upon the ſubject. My inquiries, however, have been without effect. I cannot find that any particular obſervation had been made upon the pulſe by the phyſicians who attended him. It is not fair to conclude from thence, that the action of the heart is exactly the ſame whether it be incloſed or not in its proper membrane; but it is ſurely enough to ſhew that the action is not very much affected by the want of it. The different conſiderations which have been mentioned above, will ſerve, I hope, in ſome meaſure to explain this effect, and to point out how the circulation may be carried on, even for a very long period, under theſe particular circumſtances.

EXPLANATION

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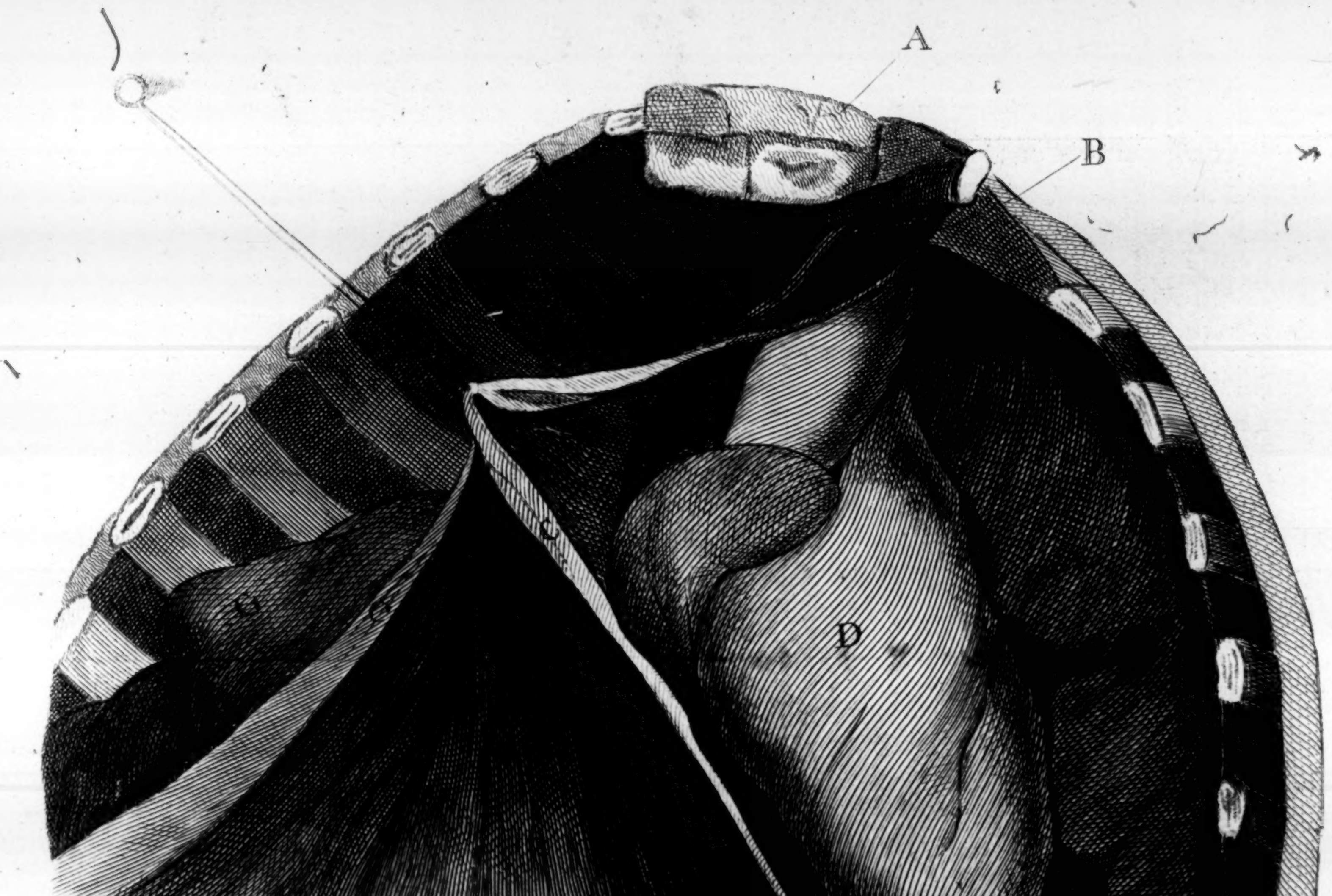
A
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VII. *On* INTROSUSCEPT
HUNTER, Eſq. F. R.
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to the Army. Read Aug

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SCEPTION. *By* JOHN
F. R. S. *Surgeon Extra-*
ing, and Surgeon General
ad Auguſt 18, 1789.

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a looſe inteſtine being contracted, and the part immediately below relaxed and dilated; under which circumſtances it might very readily happen, by the contracted portion ſlipping a little way into that which is dilated, not from any action in either portion of inteſtine, but from ſome additional weight in the gut above. How far the periſtaltic motion, by puſhing the contents on to the contracted parts, may force that into the relaxed, I will not determine, but ſhould rather ſuppoſe that it would not.

By this mode of accounting for an accidental introſuſception, it may take place either upwards or downwards; but if a continuance or an increaſe of it ariſes from the action of the inteſtine, it muſt be when it is downwards, as we actually find to be the caſe; yet this does not explain thoſe in which a conſiderable portion of inteſtine appears to have been carried into the gut below: to underſtand theſe, we muſt conſider the different parts which form the introſuſception. It is made up of three folds of inteſtine; the inner, which paſſes down, and being reflected upwards, forms the ſecond or inverted portion, which being reflected down again, makes the third or containing part, that is the outermoſt, which is always in the natural poſition.

The outward fold is the only one which is active, the inverted portion being perfectly paſſive, and ſqueezed down by the outer, which inverts more of itſelf, ſo that the angle of invertion in this caſe is always at the angle of reflection of the outer into the middle portion or inverted one, while the innermoſt is drawn in. From this we can readily ſee how an introſuſception, once begun, may have any length of gut drawn in.

The external portion acting upon the other folds in the ſame way as upon any extraneous matter, will, by its periſtaltic motion, urge them further; and, if any extraneous ſubſtance is detained in the cavity of the inner portion, that part will become a fixed point for the outer or containing inteſtine to act upon. Thus it will be ſqueezed on, till at laſt the meſentery preventing more of the innermoſt part from being drawn in, will act as a kind of ſtay, yet without intirely hindering the inverted outer fold from going ſtill further. For it being the middle fold that is acted upon by the outer, and this action continuing after the inner portion becomes fixed, the gut is thrown into folds upon itſelf; ſo that a foot in length of inteſtine ſhall form an introſuſception of not more than three inches long.

The different appearances which I have deſcribed as taking place in an introſuſception, are diſtinctly ſeen in the annexed plate, in which the different folds of inteſtine are expoſed.

I have aſſerted that the outer portion of inteſtine was alone active in augmenting the diſeaſe when once begun; but if the inner one was capable of equal action in its natural direction, the effect would be the ſame, that of endeavouring to invert itſelf, as in a prolapſus ani; the outer and inner portions, by their action, would tend to draw in more of the gut, while the intermediate part only would, by its action, have a contrary tendency.

The action of the abdominal muſcles cannot aſſiſt in either forming, or continuing this diſeaſe, as it muſt compreſs equally both above and below, although it is capable of producing the prolapſus ani.

In caſes where introſuſception begins at the valve of the colon, and inverts that inteſtine, we find the ilium is not at all affected;

which proves that the meſentery, by acting as a ſtay, prevents its inverſion.

From the natural attachment of the meſentery to the inteſtines, one would, at the firſt view of the ſubject, conceive it impoſſible for any one portion of gut to get far within another; as the greater extent of meſentery that is carried in along with it would render its further entrance more and more difficult, and we ſhould expect this difficulty to be greater in the large inteſtines than in the ſmall, as being more cloſely confined to their ſituation; yet the largeſt introſuſception of any known was in the colon, as related by Mr. Whately*.

The introſuſception appeared to have begun at the inſertion of the ilium into the colon, and to have carried in the cæcum with its appendix. The ilium paſſed on into the colon, till the whole of the aſcending colon, the tranſverſe arch, and deſcending colon, were carried into the ſigmoide flexure and rectum. The valve of the colon being the leading part, it at laſt got as low as the anus; and when the perſon went to ſtool he only emptied the ilium, for one half of the large inteſtines being filled up by the other, the ilium alone, which paſſed through the center, diſcharged its contents.

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* Vid. Philoſ. Tranſ. Vol. lxxvi. page 305.

Since that time the following caſe has occurred, which is in many reſpects ſimilar; the patient was attended by Dr. Aſh, and the body inſpected after death by Mr. Home. A. B. aged nine months, a large healthy well-looking child, who, as far as appeared, had never been indiſpoſed from his birth, was ſeized with a ſtrong ſpaſm, ſtretching himſelf out ſuddenly, without having had any ſymptoms of previous ailment. Either during the ſpaſm, or immediately after it, he paſſed a very large looſe ſtool, and after that diſcharged, at intervals, ſmall quantities of mucous ſlime, covered over with little ſpecks of recent fluid blood. Dr. Aſh viſited him four or five hours after this attack, and found him in all other reſpects perfectly well; the child ſucked heartily, but Dr. Aſh, on obſerving his pulſe to be leſs quick than is uſual in children ſo young, his heat to be rather below the common ſtandard, and, added to theſe, the ſmall mucous and bloody diſcharges, ſuſpected that mortification had taken place in the bowels, without being able to gueſs at the cauſe, as the child had laboured under no previous indiſpoſition. In this uncertain ſituation, various means of relief were attempted by purgatives, fomentations, the warm bath, and different kind of clyſters, but without any good effect. On his firſt examination of the abdomen he felt (or thought he felt) a deep-ſeated fullneſs or hardneſs under the left hypochondrium; bliſters were applied to the part, and every poſſible means attempted, without obtaining any evacuation by ſtool, or any other apparent relief; his ſtrength gradually ſunk, and his pulſe became gradually weaker, although he continued to take the breaſt eagerly till within a few hours of his death, which happened juſt ſixty hours after the firſt ſpaſmodic attack.

The following were the appearances found in the dead body. Upon opening the abdomen, the ſmall inteſtines conſiderably diſtended with fluid contents, occupied ſo much of the cavity as to prevent any of the other viſcera from being ſeen; and the meſentery was ſo much confined, that the convolutions of the ſmall inteſtines could not be readily followed. This confinement was found to ariſe from an introſuſception of the ilium and its meſentery, together with the cæcum, and aſcending colon into the deſcending part of the ſigmoide flexure of the colon, the meſentery of the ilium being drawn up ſo obliquely acroſs the root of the meſentery, as to prevent the jejunum from having its uſual freedom of attachment.

The only part of the colon which could be ſeen was the ſigmoide flexure, in which was diſtinctly to be felt a hard ſubſtance, conſiſting of the ilium and inverted colon. Theſe parts being removed, for the purpoſe of a more accurate inſpection of them, the ſigmoide flexure of the colon was laid open, and was diſcovered to contain the cæcum, and colon in an inverted ſtate. The internal ſurface of theſe, when expoſed, were found to have put on a dark red appearance, approaching to black; the whole appearing like a ſolid ſubſtance, rounded at the end, hanging looſe into the deſcending colon, and about four inches long. Upon dividing the inverted colon, the ilium and appendix cæci were ſeen lying cloſe to each other, and their two openings found on the rounded end of the inverted colon, leading directly into the ſigmoide flexure; the portion of the ilium was a little twiſted, but not in the leaſt corrugated, it was rather ſtretched, and much preſſed againſt the appendix cæci and its own meſentery, by the ſurrounding colon, and a convolution of the appendix near the termination was ſo much preſſed againſt the ilium as to make a mark upon it, and probably had compreſſed its ſides ſo as to prevent any thing from paſſing. The portion of ilium was about four inches long.

The inverted colon had drawn in the meſocolon, and a portion of the omentum that was attached to the tranſverſe arch. The portion of the colon near the valve, which formed the extremity of the inverted part, was much thickened in its ſubſtance by the effects of inflammation, being four or five times its natural thickneſs; it was a good deal corrugated, or folded upon itſelf, the folds at this part ſeemed to adhere to one another, and form one maſs. The inflammation and thickening only extending two inches, the gut becoming gradually thinner till it was of its natural thickneſs and appearance; ſo that what was only four inches in length of introſuſception, contained a conſiderably greater length of inteſtine.

The ſigmoide flexure, which was the containing inteſtine, had the natural appearance, but was dilated or relaxed, and the other contents of the abdomen were in a natural ſtate, nor had the child any other apparent diſeaſe. A repreſentation of the parts is annexed.

From the account I have given of introſuſception, it does not ſeem probable that it ſhould be of the retrograde kind, unleſs from an inverſion of the periſtaltic motion, which could only continue for a ſhort time, and the natural motion being reſtored, a cure would probably be performed.

This diſeaſe happens moſt frequently in the firſt fifteen years of life, not occurring ſo commonly in older people, neither does it, I believe, ever take place in the colon itſelf, although we find that gut affected by it *.

An introſuſception can never be perfectly known till after death; but where there are violent affections of the bowels, attended with conſtipation, we have reaſon, from the caſes

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* A prolapſus ani is, in ſome reſpects, ſimilar to an introſuſception, and may poſſibly begin in the ſame way, but is continued by the action of the abdominal muſcles, never by the action of the gut itſelf. It differs from introſuſception as not being contained in a gut; for, inſtead of having an incloſing gut inverting itſelf by its own action, there is an incloſed gut protruded by the action of the abdominal muſcles, and the paſſing of the fæces through it, and the point of inverſion is at the extremity of the protruſion, and as it inverts it puſhes out of the body.

which have been examined in the dead body, to ſuppoſe that this diſeaſe may be the cauſe of them: there are, however, ſo many other diſeaſes which produce the ſame ſymptoms, that nothing can be aſcertained. But if an introſuſception is ſuſpected, it will be proper, in the mode of treatment, to ſuppoſe it to be of the progreſſive kind.

In the treatment of this diſeaſe various methods have been propoſed; bleeding, to leſſen the inflammation that might be brought on, and quickſilver to remove the cauſe, are the moſt obvious, and the means that are uſually recommended.

Quickſilver would have little effect either in one way or the other, if the introſuſception was downward; for it is to be ſuppoſed that it would eaſily make its way through the innermoſt contained gut, and, if it ſhould be ſtopped in its paſſage, it would, by increaſing its ſize, become a cauſe (as before obſerved) of aſſiſting the diſeaſe. In caſes of the retrograde kind, quickſilver, aſſiſted by the periſtaltic motion, might be expected to preſs the introſuſception back; but even under ſuch circumſtances it might get between the containing and inverted gut into the angle of reflection, and, by puſhing it further on, increaſe the diſeaſe it is intended to cure.

From the account I have given of the manner in which it is produced, I ſhould propoſe the following treatment in caſes of progreſſive introſuſception.

Every thing that can increaſe the action of the inteſtine downwards is to be particularly avoided, as tending to increaſe the periſtaltic motion of the outer containing gut, and thus to continue the diſeaſe. Medicine can never come in contact with the outer fold, and, having paſſed the inner, can only act on the outer below, therefore cannot immediately affect that portion of the outer which contains the introſuſception; but we muſt ſuppoſe that whatever affects, or comes in contact with the larger portion of the canal, ſo as to throw it into action, will alſo affect by ſympathy any part that may eſcape ſuch application. I ſhould therefore adviſe giving vomits, with a view to invert the periſtaltic motion of the containing gut, which will have a tendency to bring the inteſtines into their natural ſituation.

If this practice ſhould not ſucceed, it might be proper to conſider it as a retrograde introſuſception, and, by adminiſtering purges, endeavour to increaſe the periſtaltic motion downwards.

SUPPLEMENT *to the Paper upon* INTROSUSCEPTION. *By Mr.* HOME.

THE following caſe of introſuſception upwards has been communicated to me by the late Mr. Smith, ſurgeon at Briſtol; in whoſe collection of morbid preparations I ſaw the parts which had been removed from the dead body, and preſerved in ſpirits. That the introſuſception was upwards, is ſufficiently aſcertained from the preparation.

The particulars of the caſe, as far as they came to the knowledge of Mr. Smith, who examined the body after death, were as follow:

A cabin-boy belonging to one of the ſhips lying in Briſtol harbour, was corrected by his maſter for ſome miſconduct, at which the boy was very much exaſperated, and, in the heat of his paſſion, ſwallowed ſome arſenic, which had been laid in different parts of the ſhip to poiſon rats. He was attacked with violent pain in the ſtomach and inteſtines, attended with exceſſive vomiting, and expired before any medical aſſiſtance could be procured.

Upon inſpection of the body, the internal ſurface of the ſtomach was found inflamed to a very great degree, the inflammation extending a conſiderable way along the tract of the ſmall inteſtines, and in the ilium there was diſcovered an introſuſception of above two inches long, formed by a portion of the lower part of the gut having been inverted, and puſhed into that immediately above it.

On ſlitting up the inteſtine, to examine the introſuſception more accurately, a long round worm was found coiled round the projecting introſuſcepted part, and is preſerved in that ſituation in the preparation.

The circumſtances under which the round worm was found, determines the kind of introſuſception, ſince I have obſerved that ſpecies of worm more commonly in the upper portion of the ſmall inteſtines, in which ſituation having been diſturbed by the effects of the arſenic, it had conſequently moved, to get at a greater diſtance from the poiſon. It is likewiſe probable, from the coming on of inflammation, that vomiting, an immediate effect of the poiſon, by which the periſtaltic motion was inverted, and the introſuſception produced, had been followed by a contrary action, or diſpoſition to purging, by which the worm being hurried along till it came to the introſuſcepted part, was there ſtopped, and, in the endeavour to extricate itſelf, had twined round the projecting part, in which ſituation it died. If the introſuſception had been downwards, the worm could not have been in that ſituation.

It is probable, that if the boy had outlived the immediate effects of the arſenic, and the periſtaltic motion had perfectly recovered itſelf, the introſuſception would have been by that means unfolded.

EXPLANATION *of* PLATE III.

a. THE ilium paſſing into the introſuſception.

b. The portion of the ilium included in the introſuſception.

c. The termination of the ilium in the valve of the colon; from which a bougie paſſes into the inteſtine.

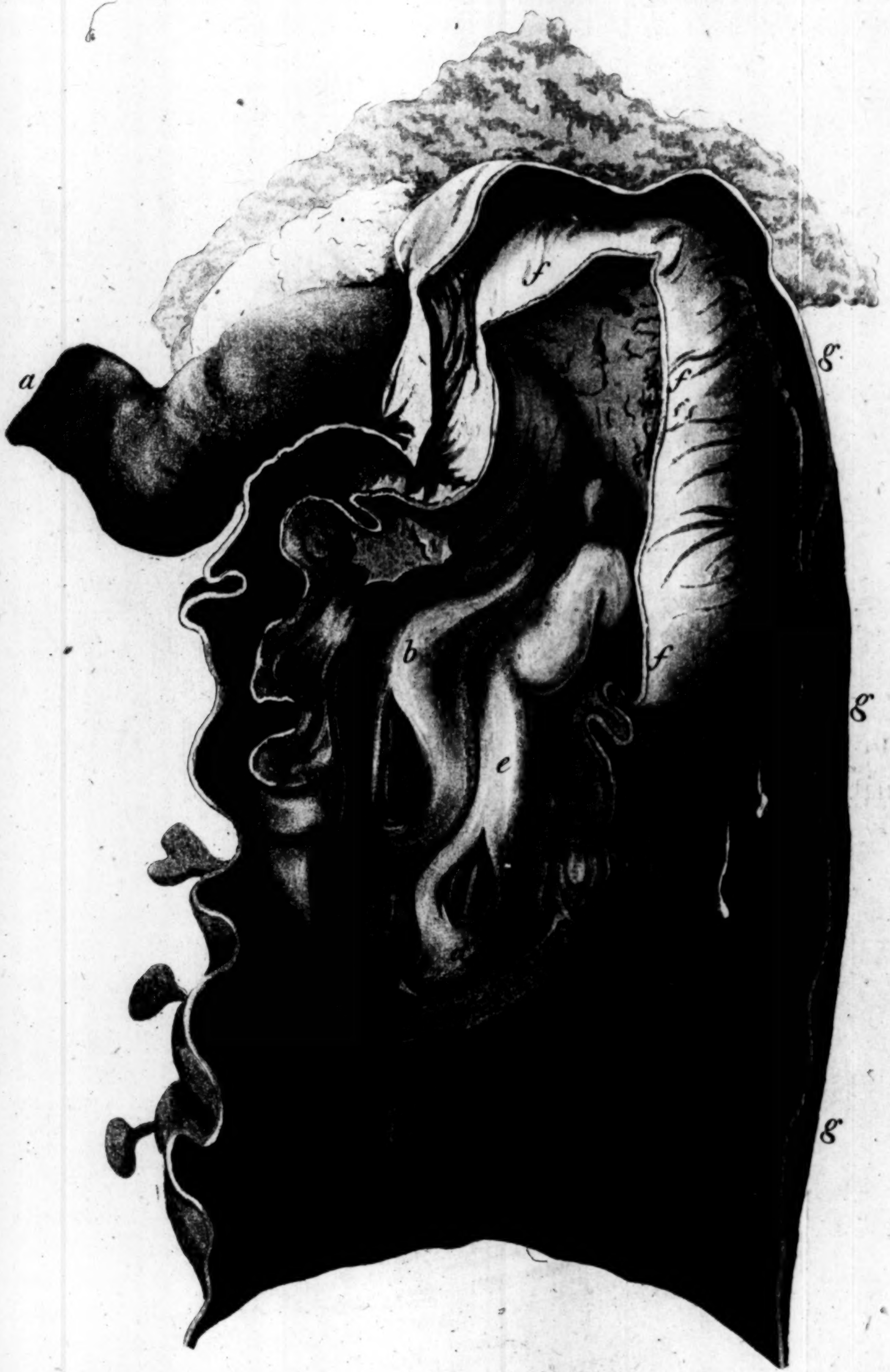
d. The orifice of the appendix cæci with a briſtle introduced into it.

e e. The courſe and termination of the appendix cæci.

f f f. The inverted or contained portion of the colon, the inner ſurface of which has portions of coagulable lymph adhering to it, the conſequence of previous inflammation.

g g g. The containing colon laid open, to expoſe the introſuſception. Upon its external ſurface are the appendiculæ epiploicæ.

Plate 3, p.118.



VIII. *Of uncommon Appearances of Diſeaſe in Blood-veſſels.* *By* MATTHEW BAILLIE, M.D. F.R.S. *and Phyſician to St. George's Hoſpital.* *Read September* 15, 1789.

IT is of conſequence to remark ſingular appearances of diſeaſe in the body, even if they ſhould not obviouſly lead to any uſeful obſervations in practice; becauſe they open a more extended view of the operations of the animal œconomy, point out more clearly the reſources of nature, and render the invention of the mind more generally fitted to aſſiſt, when her efforts would otherwiſe be ineffectual. It is in this point of view that I think the following remarks upon ſome uncommon appearances of diſeaſe in blood-veſſels, may not altogether be unworthy of notice. Two of the appearances which I ſhall deſcribe are certainly very rare, and the other by no means common.

It is known to every perſon who is acquainted with the animal œconomy and pathology, that the blood coagulates, under certain circumſtances, in the veſſels of the living body. One caſe in which it coagulates is, when a ligature has been made upon a veſſel, and the blood is prevented from flowing thro' it at that part, as in the artery of an amputated limb. Another caſe in which it coagulates is, when a veſſel is dilated at any part into a ſort of bag, ſo that the blood there moves ſlowly, and as it were out of the courſe of the circulation. The coagulum begins to be formed at the greateſt diſtance from the current of blood; or, in other words, cloſe to the ſides of the dilated bag at the greateſt diſtance from the cylindrical cavity of the artery. It is very rare that a coagulum is formed in an aneuryſmal artery, until it is conſiderably enlarged beyond its ordinary ſize, and the blood is a good deal removed from its natural courſe of circulation. It is ſtill more rare that the coagulum fills up the whole cavity in which it is formed, ſo as to prevent the circulation altogether through that part of the veſſel. Were this laſt circumſtance often to take place, it would frequently become the natural cure of aneuryſm, and ſuperſede the very painful means of a doubtful operation. It ſometimes happens, however, that nature deviates from the ordinary courſe of diſeaſed operation, and forms a coagulum ſo complete, as to fill up entirely the cavity in which it takes place, without there having been any previous ſtoppage to the circulation by a veſſel being rendered impervious, and where a veſſel is not much enlarged beyond its ordinary ſize. I had an opportunity of obſerving this in the carotid artery of a man, who was brought, about two winters ago, to Windmill-ſtreet for diſſection. The whole of the arterial ſyſtem in this man had a tendency to aneuryſm, the natural ſtructure being in many places changed, and the diameter of the arteries being ſomewhat enlarged beyond their natural ſize. There was, however, no part dilated into a bag or ſack, containing a coagulum, except, as I ſhall immediately explain, in the carotid arteries.

In the right carotid artery, juſt before it divides into the external and internal carotids, I found an oval uniform ſwelling, about an inch and an half in length, and the diameter of the artery was ſcarcely enlarged to more than twice its ordinary ſize. The ſwelling was firm, giving the ſame reſiſtance to the feeling as a healthy abſorbent gland, and, if it had been felt through a thin layer of muſcle, would certainly have been miſtaken for one of a large ſize. I mention this compariſon, to give a more diſtinct idea of the tumor. When I cut through the coats of the artery, I found its cavity completely filled with a firm coagulum of blood, which had not the appearance of blood being recently coagulated after death, as in the beginning of the pulmonary artery, but had the appearance of an old aneuryſmal coagulum.

The coagulum adhered every where ſo firmly to the inſide of the veſſel, that, in ſeparating it, the inner coat was, in many places, peeled off along with the coagulum. In cutting into its ſubſtance, I found it conſiſting of diſtinct layers, as in a common aneuryſm. There was no part of it which had the appearance of being recently formed, and, therefore, there cannot be any doubt of its having exiſted for a conſiderable time before the man's death. It is obvious then, that in this caſe a coagulum had been formed in the carotid artery, undergoing the ſame proceſs as in aneuryſm, and that the tendency to aneuryſm had remedied itſelf. The whole cavity being filled up with the coagulum, there was no circulation whatever at this part; hence the cauſe of further dilatation was removed, and there was no danger of the rupture of the veſſel, which is the principal danger in this diſeaſe.

A few caſes only have been related by authors of aneuryſms being cured without any chirurgical operation, and even ſome of theſe have been ſuſpected not to be authentic. Two caſes have occurred lately to Mr. Ford, ſurgeon, deſcribed by him in the London Medical Journal, about which there can be no doubt. Mr. Home has offered a very ingenious explanation of ſuch caſes, founded on obſervations made by Mr. Hunter on the ſtate of arteries in mortification, viz. that it is probable the blood coagulates in the artery above the ſeat of the aneuryſmal bag, ſo as to render it there impervious. This explanation, although it accounts ſufficiently for the caſe to which it immediately refers, yet is not applicable to all caſes where aneuryſms undergo a natural cure. In the one which I have related there was no coagulum of blood formed except in the dilated part of the artery, no ſteps whatever towards mortification had taken place, ſo that the natural cure depended entirely on the coagulum formed where the artery was enlarged. The blood here had coagulated much more readily than in proportion to the dilatation of the artery, ſo that the whole cavity was filled up before it had enlarged to any conſiderable ſize. Wherever there is a diſpoſition in the blood to coagulate greater than in proportion to the enlargement of the artery, the coagulum will at length fill up the whole cavity of the dilated part, and produce a natural cure of aneuryſm. This ſtate of the blood, or rather of the coagulable lymph, may ariſe from ſome connection or ſympathy it may have with the diſeaſed ſtructure of the artery, but it is perhaps impoſſible to determine this with certainty.

We may remark, that in the left carotid artery of the ſame perſon, exactly before its diviſion into the external and internal carotids, a dilatation and coagulum were formed; but the dilatation had more the ſhape of a common aneuryſmal ſack, and the coagulum did not entirely fill up the cavity of the veſſel. There was only, however, a ſmall canal for the current of blood, and I am inclined to think that the whole cavity would very ſoon have been filled up, ſo that on this ſide alſo there would have been a natural cure for the aneuryſm.—From the ſituation of the two carotid arteries I ſhall juſt beg leave to obſerve, that it is not improbable a perſon might live without circulation through a part of the main trunks of both carotid arteries, ſo that if it ſhould become abſolutely neceſſary in any chirurgical operation, they might be taken up by ligature. Mr. Hunter, in his Lectures upon Aneuryſm, has mentioned nearly the ſame opinion *.

Of the OBLITERATION *of* VESSELS.

IT is well known, that under certain circumſtances both arteries and veins become changed in their ſtructure, loſing their cavities, and degenerating into a ſort of ſpungy ligamentous ſubſtance. This happens when it is no longer neceſſary that the blood ſhould circulate in them, as in certain changes in the circulation of an animal at a particular period of life. When a child is born, it is neceſſary that the whole blood of the body ſhould be carried through the lungs, in order to undergo a change which is intimately connected with life, and that there ſhould be no other communication between the blood circulating in the pulmonary artery and in the aorta, than through the lungs and the left ſide of the heart. Hence the ductus arterioſus gradually contracts itſelf after the birth of the child, till at length it becomes a ſort of ligamentous ſubſtance *, without any cavity. The time which is occupied by this proceſs is not long, although it varies in different perſons, and in ſome few inſtances the veſſel has been found in the adult ſtate not entirely obliterated. In proportion as the ductus arterioſus contracts itſelf, the two branches of the pulmonary artery become enlarged, till at length they are able to convey to the lungs the whole of the blood which is thrown out by

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* This opinion is in ſome meaſure confirmed by an experiment made by Valſalva, where he tied up both carotid arteries of a dog, which lived for two and twenty days afterwards, and might have continued to live, but that he was killed for the purpoſes of diſſection. In two other experiments of the ſame ſort the dog lived a much ſhorter time; in one inſtance three days, and in another ſix.——Vid. Valſalv. Opera, Cura Morgagni, Epiſt. xiii. p. 507.

It would require a great many experiments to aſcertain the common event of ſuch an operation; but from what has been ſaid it appears very obvious that it is capable of ſucceeding, and it could never be propoſed by any perſon of common underſtanding, except as the only means left of ſaving a patient's life.

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* Although I have mentioned here, and in other parts of this paper, that blood-veſſels degenerate into a ſort of ligamentous ſubſtance, the expreſſion is not perfectly accurate. When their cavities are obliterated, they reſemble a good deal ligament, but they ſtill retain many properties of ſtructure which belonged originally to them, eſpecially their elaſticity.

by the right ventricle of the heart, and in this way the full circulation through the lungs is completed. In the ſame manner the umbilical arteries and vein degenerate into a ligamentous ſubſtance when all communication between the child and placenta has ceaſed, and when the blood is to be thrown in different channels for the growth of the child. It ſometimes happens, however, (although I believe it to be extremely rare) that there is a proceſs of obliteration without any of theſe circumſtances, depending upon ſome diſpoſition in the veſſels, which we cannot explain.—Some years ago a woman was brought to Windmill-ſtreet for diſſection, in whom the vena cava inferior was found to be changed into a ligamentous ſubſtance, from the entrance of the emulgent veins even to the right auricle of the heart. The cavity here was ſo entirely obliterated, as not only to prevent all circulation of blood through this part of the vein, but even in a great meaſure to prevent the admiſſion of air by inflation. It was in an attempt by Dr. Hunter to inflate the vein from the lower extremity which made this peculiarity be diſcovered. The blood being prevented from paſſing thro' the vena cava inferior, flowed into the lumbar veins, enlarging them gradually as that vein became contracted, till at length they were of a ſufficient ſize to receive the whole blood which returns by the vena cava. From the communication between the lumbar veins and the vena azygos, the blood paſſed into this vein, and was conveyed to the heart. It happened in this particular inſtance that there was an additional vena azygos, upon the left ſide of the ſpine, ſo that the blood was conveyed more readily to the heart than if there had been one vena azygos, as in ordinary caſes. The enlarged veins were in ſome places thrown into varices, as muſt naturally take place under the circumſtances we have mentioned. What time was conſumed in the obliteration of the vena cava inferior, it is impoſſible to ſay; but, if we conſider the proceſs as analogous to the obliteration of the ductus arterioſus, and the umbilical arteries and vein, it could not be long; we ought not, however, to reſt a very ſtrong opinion upon this analogy *.

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* We ought not to reſt any ſtrong opinion upon this analogy, becauſe it is eaſy to ſee a reaſon why the obliteration of the cavity in the ductus arterioſus and umbilical arteries ſhould be a ſhort proceſs; but there is not the ſame reaſon for its being a ſhort proceſs in the obliteration of the vena cava inferior, which we have juſt deſcribed.

This caſe ſhews, that, on ſome occaſions, a proceſs of obliteration takes place in the blood veſſels, independent of the natural circumſtances formerly explained. It ſhews alſo the very great reſources of nature, that it can bear the function of the largeſt vein in the body to be ſuſpended without endangering the life of the animal; for it cannot be doubted that the obliteration we have mentioned had no ſhare in producing the woman's death. The blood ſtill circulated in the common quantity to and from the heart, but at one place it took a circuitous road inſtead of a direct one. I think the natural reſources of the body in this particular to be ſo great, that I ſhould not heſitate to believe, if the aorta was to be obliterated in any part of its paſſage between the origins of the ſuperior meſenteric and inferior meſenteric arteries, that the circulation could be carried on by the communicating branches of theſe two veſſels, together with the ſmaller aids of the anaſtomoſes in the lumbar arteries.

It is reaſonable to think, that where an obliteration takes place in any artery or vein, there are pretty large collateral veſſels; where there are none, or very few collateral branches, the difficulty of the blood's being impelled into other channels, would probably prevent the obliteration from taking place. In the natural obliterations we find, that there are always in the neighbourhood veſſels of conſiderable ſize. Thus near the ductus arterioſus are the two large branches of the pulmonary artery; near the umbilical arteries are the large and numerous branches of the internal iliacs; and near the umbilical vein are the large branches of the vena portarum. In the ſame manner, in diſeaſed obliteration of veſſels, the collateral branches muſt be ſufficiently large to carry off the current of blood[*][†].

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[*] Morgagni mentions an inſtance of the ſame ſort of obliteration in veins, and conſiders it as extremely ſingular.

"Iliaca igitur vena dextra æquo erat latior, at ſiniſtra ejuſque rami tenues adeo & pallentes ut rem nunquam antea a me viſam miratus cavæ imum truncum aperuerim. Quo facto vel magis miratus ſum. Nam pro Iliacæ ejus venæ oſtio lineam inveni ipſius coalitum indicantem, qua in linea duo aut tria parva erant oſcula cum iliaca vena communicantia. Quam cum ſecundum longitudinem incidiſſem & præter filamenta polypoſa quendam fibrarum quaſi faſciculum introrſum exſtantem conſpexiſſem, mox facile animadverti venæ parietes ab uno latere inter ſe coaluiſſe, ſicque eum faſciculum mentiri cujus ſpecies manu utraque parietes diſtrahendo, neque enim perdiffi cilis erat ſejunctio, prorſus evaneſcebat."—Vid. Epiſt. 56. Artic. 10.

[†] I have had an opportunity alſo of ſeeing another inſtance of obliteration in veins, very ſimilar to what I have deſcribed, at the Lyceum Medicum Londinenſe.

In writing upon this ſubject, it may be worth while to mention, that I once found a ſtricture in the femoral artery of an adult ſubject, juſt under the origin of the arteria cruralis profunda. There was a contraction of the artery ſo complete, that at one part the ſides were nearly in contact, but, as far as I could judge, without any diſeaſed alteration of ſtructure. This took place where conſiderable branches are given off, by which the blood could be readily conveyed to the femoral artery, beyond the place of contraction. What could be the cauſe of this contraction it is not eaſy to imagine; it certainly did not ariſe from any mechanical conſtriction, and therefore would ſeem to depend upon ſome action in the coats of the veſſel itſelf.

Let us ſuppoſe that an aneuryſm had been previouſly formed in the femoral or popliteal artery of this particular limb, what would have been the conſequence? Would this contraction of the artery near the groin prove a natural cure of the aneuryſm? There is certainly a great analogy between this natural proceſs of contraction in the artery, and the operation for the aneuryſm as lately performed by Mr. Hunter. This, in the operation for the popliteal aneuryſm, conſiſts in ſimply making

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an inciſion upon the femoral artery before it paſſes through the tendon of the triceps muſcle, and taking it up there by a ligature, which promiſes to be a great improvement in the art of ſurgery. There is this difference, however, between them, that when a ligature is made upon an artery, the blood is prevented all at once from paſſing through it, and it takes a very conſiderable time before the collateral branches enlarge, ſo as to convey the blood in any quantity to the principal artery: beſides, the ligature is made ſo near the aneuryſm, that the enlarged collateral branches communicate principally with others ariſing beyond it. In the caſe we have mentioned, the contraction of the artery was probably a gradual proceſs, the collateral branches enlarging in proportion as the contraction encreaſed, ſo that the ſame quantity of blood was always circulating in the limb. But ſtill the impetus of the blood muſt have been greatly diminiſhed by paſſing through a number of meandring channels, inſtead of one ſtraight canal; ſo that if the progreſs of an aneuryſm could not be ſtopped by it, it would at leaſt advance very ſlowly; and if an aneuryſm had been formed near the ſeat of the contraction, it muſt have been as effectually cured by it as by an operation *.

Of the OSSIFICATION *of* VESSELS.

THERE is no diſeaſed change of ſtructure more common in the body, than partial oſſifications of the arterial ſyſtem at an advanced period of life. The time of life at which this proceſs begins, and its progreſs, vary very much in different perſons. There is alſo conſiderable difference in the ſort of change. In ſome there is a converſion of the coats of an artery into a ſubſtance, reſembling very much the common bony matter; in others there is a converſion of them into an earthy matter, little blended with the animal gluten. This ſort of change is ſo common, that when a perſon dies about the age of ſixty years, it is more frequently found to have taken place in

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[*] There is an inſtance of this ſort of contraction in arteries mentioned by Morgagni.

"Sed illud peculiare quod ſi eandem faciem (ſcil. interiorem aortæ) inſpiceres, tunicæ latus arteriæ ſiniſtrum & poſteriorem partem facientes ad angulum inter ſe jungi, non curvæ inſtar lineæ videbantur."—Vid. Lib. v. Epiſt. 65. Artic. 5.

in a greater or leſs degree, than that the arterial ſyſtem poſſeſſes its original healthy ſtructure. The venal ſyſtem, on the contrary, is ſo rarely liable to this ſort of change, that it may be ſaid to be a diſeaſe in it almoſt entirely unknown. There is one inſtance, however, in the collection of Windmill-ſtreet, where a conſiderable oſſification was formed in the coats of the vena cava inferior, near its bifurcation into the two iliacs *. Why there ſhould be this difference in the coats of arteries and veins, is not eaſy to explain. There ſeems to be no reaſon for it *à priori*. It might, perhaps, be ſaid, that it depends on the vaſa vaſorum being more numerous on the coats of arteries than of veins, but it certainly does not depend upon this circumſtance. The vaſa vaſorum are more numerous on the coats of arteries in younger than in older people, yet in the former oſſification ſcarcely ever takes place.—Beſides, it is obvious, that a peculiar effect muſt depend on a peculiar action, and not ſimply upon the number of veſſels *. I do not, therefore, pretend to explain why oſſification ſhould be ſo common an occurrence in the coats of arteries, and ſo rare in thoſe of veins, but I know it to be a very rare occurrence in the latter, and for this reaſon I thought it worthy of mentioning, as a ſingular matter of fact in the hiſtory of diſeaſed appearances †.

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* This oſſification was found in the ſame vein which had the extraordinary obliteration we have deſcribed in the former part of this paper.

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* Bony matter, whether depoſited in the coats of arteries or veins, can only be formed by the vaſa vaſorum ſecreting bony matter in the ſame manner as it is ſecreted in the growth of bones generally.

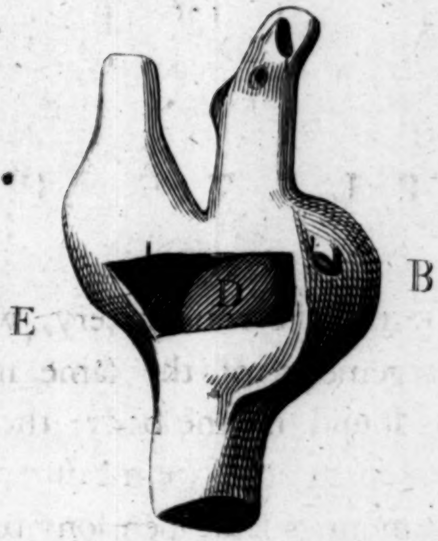
† When oſſification takes place in the valvulæ tricuſpides, or mitrales, it perhaps may be conſidered as an oſſification belonging to the venal ſyſtem, but even this is very rare.

PLATE IV.

A. THE right carotid artery, with its enlargement, of the ſame ſize exactly as found in the body, the ſize being taken by accurate meaſurement. The tumour is laid open longitudinally, to ſhew that it is entirely filled with coagulable lymph.

B. The left carotid artery—D repreſenting the quantity of coagulable lymph in it, E repreſenting the proportion of cavity for carrying on the circulation.

Plate 4, p. 136.



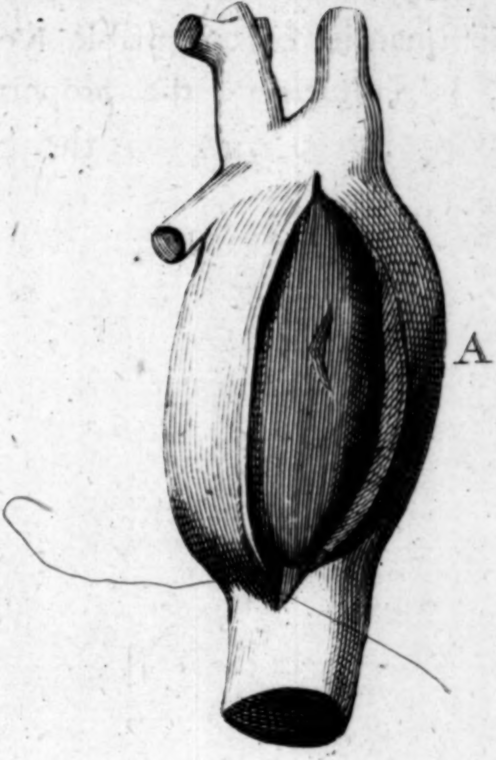
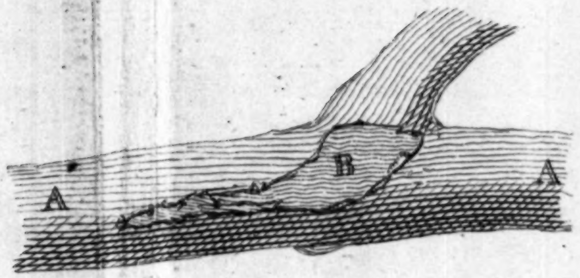


Plate 5. p. 337.



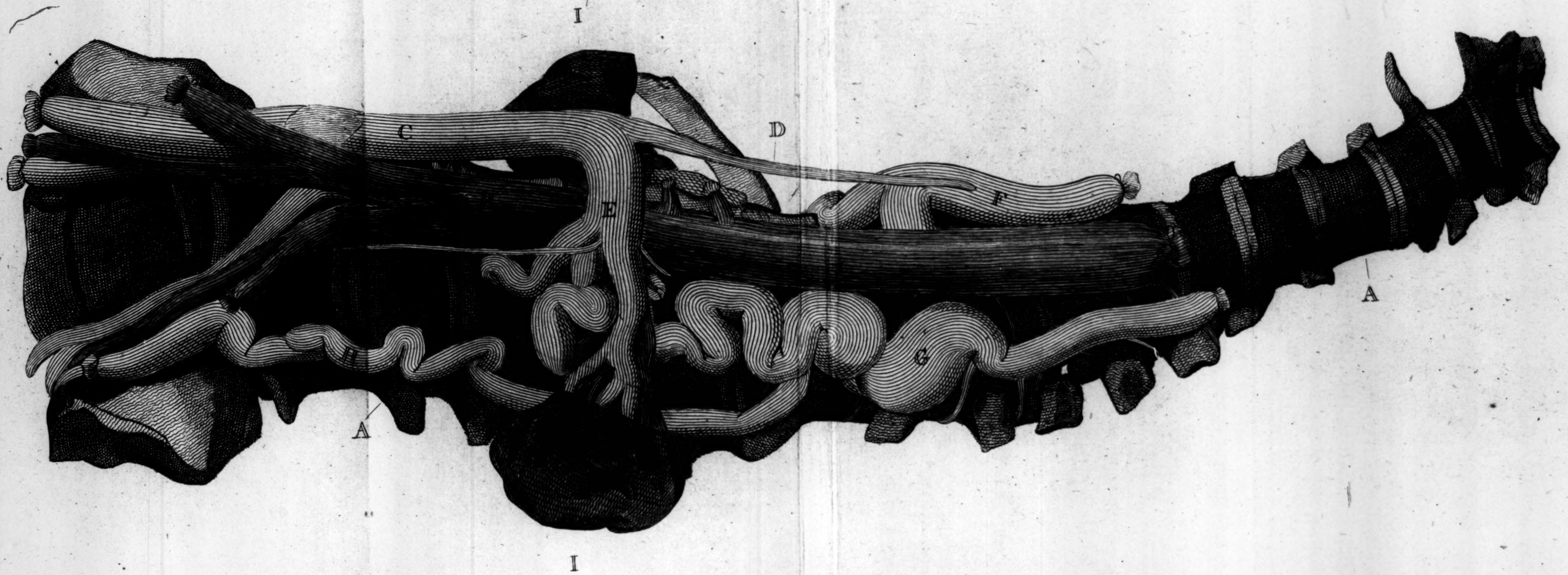


PLATE V.

FIG. I.

AA. Spine.
BB. Aorta, with the iliac branches.
C. The vena cava inferior, with a white oſſified part, more fully ſhewn in Fig. 2.
D. The obliterated portion of the vein.
E. Left emulgent vein enlarged.
F. The vena azygos conſiderably enlarged beyond its uſual ſize.
G. An uncommon vena azygos of the left ſide, very much enlarged and tortuous.
H. Veins of communication between the left iliac vein and left vena azygos.—They are enlarged and tortuous.
II. The two kidneys dried and ſhrunk.

FIG. II.

AA. A portion of vena cava inferior.
B. Repreſents the ſize, ſhape and irregularity of the oſſified part.

N. B. Both of thoſe drawings were taken from a dried preparation long after it was made.

IX. *An*

IX. *An Account of Mr.* HUNTER'*s Method of performing the Operation for the Cure of the Popliteal Aneuriſm. By* EVERARD HOME, *Eſq;* F.R.S. *Aſſiſtant-Surgeon to St. George's Hoſpital.*

THE popliteal aneuryſm being a diſeaſe which frequently occurs, and generally proves fatal, unleſs ſome means are taken to prevent it; we cannot be ſurprized that it has attracted the attention, and called forth the exertions of the ableſt ſurgeons in this country to diſcover ſome method of cure.

Experience has ſhown that all the modes hitherto practiſed are exceedingly precarious, being rarely attended with ſucceſs, and the death of the patient being commonly a conſequence of a failure of the operation; a circumſtance which has led ſome ſurgeons of great eminence to prefer the amputation of the limb in all ſuch caſes.

Mr. Hunter, who has repeatedly performed the operation for the aneuryſm, finding that it in general fails, and having likewiſe obſerved that the removal of a limb ſo high up, from a perſon in health, ſeldom ſucceeds in preſerving life, (and when it does, leaves the patient diſabled,) was excited to conſider this diſeaſe with more than ordinary attention. The reſult has been a mode of practice that appears to poſſeſs many advantages over thoſe hitherto recommended, and to be an improvement in the practical part of ſurgery; at leaſt it is from this idea being ſtrongly impreſſed on my mind that I am induced to communicate it to this Society, as Mr. Hunter is too much engaged to permit his taking that taſk upon himſelf.

An aneuryſm is a preternatural dilatation of a portion of an artery, and in general it is a very ſmall part of the arterial coats which is thus affected. The dilatation is commonly on one ſide only, and, when once begun, gradually enlarges, from the force of the heart propelling the blood againſt the dilated part. Thus in time a ſac is formed, which being in ſome meaſure out of the direct courſe of the circulation, the blood, where it is at the greateſt diſtance from the channel of the artery, coagulates, and forms layers, or ſtrata, upon the inſide of this ſac. As the enlargement of the ſac depends intirely on the force with which the blood is acted on by the heart, it does not, as at firſt, continue to ſwell out at right angles from the ſide of the artery, but is increaſed in a diagonal line between that and the courſe of the artery itſelf, from the force of the blood being applied in that direction; ſo that the ſac is protruded along the outſide of the artery, and, by its preſſure upon it, obliterates, in many inſtances, the lower orifice, which communicates with the artery, and produces a total ſtagnation of the blood in the ſac.

If the coats of an artery are examined in the commencement of this diſeaſe, the firſt appearance is a loſs of the natural luſtre and tranſparency of its internal membrane; that becoming opaque, afterwards thicker, acquiring a leather-like appearance; and when the ſac becomes larger, the coats retain no longer a reſemblance to thoſe of an artery, but have more that of a membranous bag, communicating laterally with the artery by a rounded orifice, (of different ſizes,) the margin of which reſembles the internal membrane of an artery, in a thickened ſtate; but beyond that, gradually degenerates into a membranous ſubſtance.

The popliteal aneuryſm, which we are at preſent to conſider, is the diſeaſe above mentioned, affecting the trunk of the popliteal artery, which runs down between the two ham-ſtrings of the thigh. From the ſituation of the tumor, on whatever ſide of the artery the dilatation is produced, it will be diſtinctly felt in the hollow between the ham-ſtrings, and will be readily aſcertained by a pulſation to be felt in every part of the tumor; it ſeems to be one of the moſt frequent ſituations of aneuryſm; and though it may be difficult to aſcertain whether it occurs ſo commonly as in the aorta itſelf, it is certainly found oftener in this artery, than in any other branch which the aorta ſends off. This circumſtance, as far as I know, has not hitherto been accounted for, and what is rather curious, in many recent inſtances of this diſeaſe, the patients have been coachmen and poſtilions *. The popliteal aneuryſm has been in general

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* Morgagni and his friends found aneuryſms of the aorta more frequently in guides, poſt-boys, and other perſons who ſit almoſt continually on horſeback, which is attributed to the concuſſion and agitation.—Vide Letter xvii. Art. 18.

When we conſider the popliteal artery as affected by the different poſitions of the leg and thigh, and the obſtruction which the general ſuppoſed to ariſe from a weakneſs in the coats of the artery, independent of the preſence of diſeaſe; if this were true, we might reaſonably conclude, that, except in the part preternaturally dilated, the veſſel remained in a ſound ſtate, which would naturally ſuggeſt the mode of practice generally recommended, viz. opening the ſac, tying

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the circulation muſt inevitably meet with in that artery, when the limb is bent, we ſee a probable reaſon why it ſhould be more liable to diſeaſe than any of the other ramifications of the aorta; eſpecially when it is found that aneuryſms in the aorta itſelf are moſt frequent at the curve of that artery.

If this obſervation is allowed to have any weight, the reaſon will be evident why the diſeaſe ſhould occur more frequently in coachmen and poſtilions; for their knees being almoſt conſtantly in a bent ſtate, from the neceſſary exertions of their bodies in their different occupations, and from the violent motion of their horſes and carriages, the circulation muſt often be conſiderably increaſed; while the branches immediately below the popliteal artery will be in ſome meaſure obſtructed by the action of the gaſtrochnemii and ſolæi muſcles, in ſteddying the body in the ſtirrup, or againſt the foot-board of the carriage.

The unfavourable circumſtances reſpecting the popliteal artery, do not in common life ſeem of themſelves capable of producing diſeaſe, but when increaſed to a great degree, as in the occupations of coachmen and poſtilions: They, at the ſame time, from want of ſufficient exerciſe, have their legs weaker and leſs healthy than the reſt of the body; and the caſes to be mentioned appear ſtill further to prove, that theſe circumſtances may produce ſuch a ſtate of the artery at this part, as to diſpoſe to the formation of aneuryſm.

up the artery above and below it, leaving the bag to ſuppurate, and afterwards heal up like any common ſore.

Mr. Hunter finding an alteration of ſtructure in the coats of the artery previous to its dilatation, and that the artery immediately above the ſac ſeldom unites when tied up in the operation for the aneuryſm, ſo that as ſoon as the ligature comes away, the ſecondary bleeding deſtroys the patient, was led to conclude, that a previous diſeaſe took place in the coats of the artery, in conſequence of which it admitted of dilatation capable of producing aneuryſm. But not ſatisfied with the experiments on frogs, given by Haller in ſupport of the opinion that weakneſs alone was ſufficient to produce the dilatation, he reſolved to try the reſult in a quadruped, which, from the veſſels being very ſimilar in their ſtructure to thoſe of the human ſubject, would be more likely to aſcertain the truth or fallacy of Haller's opinion. That the experiment might have as much as poſſible the chances moſt likely to produce aneuryſm, the carotid artery, as being near the heart, was ſelected for that purpoſe.

Mr. Hunter having laid bare the carotid artery of a dog, for above an inch in length, having removed its external coat, and afterwards diſſected off the other coats, layer after layer, till what remained was ſo thin that the blood was plainly to be ſeen through it, left the dog to himſelf.

In about three weeks the dog was killed, and the parts examined, when it appeared that the two ſides of the wound having cloſed upon the artery, the whole of the ſurrounding parts were conſolidated, forming a ſtrong bond of union, and the artery itſelf was neither increaſed nor diminiſhed in ſize.

This experiment appeared very concluſive, as the coats of the artery were weakened to a much greater degree, without dilatation, than can ever happen from accident in the living body, independent of morbid affection: but it was objected, on the other hand, that the parts having been left to themſelves, immediately cloſed upon the weakened portion of the artery, and, being cemented together by the coagulated blood, effectually ſecured it againſt any dilatation. To try the force of this objection, I made the following experiment.

I laid bare the femoral artery of a dog, about two inches below Poupart's ligament, for about an inch in length, and diſſected off the coats, till the hæmorrhage from the vaſa vaſorum was conſiderable, and the circulating blood was diſtinctly ſeen through the internal membrane of the artery. The hæmorrhage ſoon ſtopped by expoſure, the ſurface was wiped dry, and afterwards covered with a doſſil of lint, to prevent the ſides of the wound from uniting. The dog continued very well, and the wound healed up from the bottom; after ſix weeks the dog was killed, and the artery was injected, that it might be examined with greater accuracy. It was not perceptibly enlarged or diminiſhed, and its coats at this part had recovered their natural thickneſs and appearance.

The reſults of theſe experiments confirmed Mr. Hunter in his opinion, that the artery, in caſes of aneuriſm, is in a diſeaſed ſtate; and led him to believe, that the diſeaſe often extends along the artery for ſome way from the ſac; and that the cauſe of failure in the common operation, ariſes from tying a diſeaſed artery, which is incapable of union, in the time neceſſary for the ſeparating of the ligature.

The femoral and popliteal arteries are portions of the ſame trunk, preſenting themſelves on different ſides of the thigh, and are readily come at in either ſituation; but where the artery is paſſing from the one ſide to the other, it is more buried in the ſurrounding parts, and cannot be expoſed without ſome difficulty.

In performing the operation for the popliteal aneuriſm, eſpecially when the tumor is large, the ligature is commonly applied on the artery at that part where it emerges from the muſcles. This mode of performing the operation will be found inadequate, if the diſeaſe of the artery extends above the ſac; for if the artery ſhould afterwards give way, there will not be a ſufficient length of veſſel remaining, to allow of its being again ſecured in the ham. To follow the artery up through the inſertion of the triceps muſcle, to get at a portion of it where it is ſound, becomes a very diſagreeable part of the operation; and to make an inciſion upon the fore-part of the thigh, to get at and ſecure the femoral artery,

tery, would be breaking new ground; a thing to be avoided, if poſſible, in all operations.

Mr. Hunter, from having made theſe obſervations, was led to propoſe, that in this operation the artery ſhould be taken up in the anterior part of the thigh, at ſome diſtance from the diſeaſed part, ſo as to diminiſh the riſk of hæmorrhage, and admit of the artery being more readily ſecured, ſhould any ſuch accident happen. The force of the circulation being thus taken taken off from the aneuriſmal ſac, the progreſs of the diſeaſe would be ſtopped; and he thought it probable, that if the parts were left to themſelves, the ſac, with its contents, might be abſorbed, and the whole of the tumour removed; which would render any opening into the ſac unneceſſary.

Upon this principle, Mr. Hunter performed the operation at St. George's Hoſpital.

The patient was a coachman, forty-five years of age; he was admitted into the hoſpital in December 1785, with a popliteal aneuriſm, which he had firſt perceived three years previous to his admiſſion, and had obſerved it gradually to increaſe during the whole of that period. It was ſo large as to diſtend the two ham-ſtrings laterally, and make a very conſiderable riſing between them; the pulſation was very diſtinct, and to be felt on every ſide of the tumor. The leg and foot of that ſide were ſo ſwelled as to be much thicker than the other, and were of a mottled brown colour; the ſwelling was not of the ædematous kind, but felt firm and brawny, probably from the extravaſation of coagulable lymph; the leg retained its natural ſhape, excepting that it was larger. Previous to performing the operation, a tournequet was applied upon the upper part of the thigh, but not tightened, that the parts might be left as much in their natural ſituation as poſſible.

The operation was begun by making an inciſion on the anterior and inner part of the thigh, rather below its middle, which inciſion was continued obliquely acroſs the inner edge of the ſartorius muſcle, and made large, to give room for the better performing of whatever might be thought neceſſary in the courſe of the operation. The faſcia which covers the artery was then laid bare about three inches in length, after which the artery itſelf was plainly felt. A ſlight inciſion, about an inch long, was then made through this faſcia, along the ſide of the veſſel, and the faſcia diſſected off; by this means the artery was exposed. Having diſengaged the artery from its lateral connections by the knife, and from the other adhering parts by the help of a thin ſpatula, a double ligature was paſſed behind it, by means of an eyed probe. The doubling of the ligature brought through by the probe, was cut ſo as to form two ſeparate ligatures. The artery was now tied by both theſe ligatures, but ſo ſlightly, as only to compreſs the ſides together. A ſimilar application of ligature was made a little lower. The reaſon for having four ligatures, was to compreſs ſuch a length of artery as might make up for the want of tightneſs, it being wiſhed to avoid great preſſure on the veſſel at any one part. The ends of the ligatures were carried directly out at the wound, the ſides of which were now brought together, and ſupported by ſticking plaiſter and a linen roller, that they might unite by the firſt intention.

The limb was found, ſome hours after the operation, not only to retain its natural heat, but even to be warmer than the other leg. The ſecond day after the operation, the brawny firmneſs of the leg was conſiderably diminiſhed, ed, it was become ſoft, looſe, and a good deal ſmaller, and the aneuriſmal tumor had loſt more than one third of its ſize.

Nothing could ſhew more plainly the action of the abſorbents, than the change the leg had undergone in ſo ſhort a time; the diminution of the tumor probably ariſing from the fluid blood which it contained having paſſed into collateral branches, or into the tibial artery.

The fourth day, on the removal of the dreſſings, the edges of the wound were found united through its whole length, excepting where prevented by the ligatures; there was neither pain nor tumifaction in the part; but the anueriſmal tumour was the ſame as on the ſecond day.

On the ninth day after the operation, there was a conſiderable diſcharge of blood from the part where the ligatures paſſed out; a tournequet was therefore applied on the artery above, which ſtopped the bleeding; and, although the tournequet was taken off a few hours after, no blood followed. The head of a roller was then placed upon the wound, in the direction of the artery, and over that the tournequet, which was not, however, tightened more than was thought ſufficient to take off the impetus of the blood in that portion of the artery.

On the tenth day appearances were much the ſame, only that between the compreſs and the knee there appeared a little fullneſs, like beginning inflammation. On the eleventh day this was gone off, and on the fifteenth ſome of the ligatures came away, followed by a ſmall diſcharge of matter, the tumor in the ham being leſſened. On the ſeventeenth day the parts ſurrounding the aneuriſmal tumor were more reduced, and pliable, ſo that it was diſtinctly to be felt.

About the latter end of January, 1786, ſix weeks after the operation, the patient went out of the hoſpital, the tumor at that time being ſomewhat leſſened, and rather firmer to the feel. He was ordered to come to the hoſpital once every week, and, in the mean time, to make ſome degree of preſſure, by application of a compreſs and bandage, with a view to excite the abſorbents to action, which in moſt caſes has a good effect.

About the middle of February the tumor had decreaſed, and was become ſtill firmer. March the 8th, the wound, which had cicatrized,

trized, broke out again, and the patient was taken into the hoſpital. About the 8th of April, ſome of the remaining thread of the ligature came away, and an inflammation appeared upon the upper part of the thigh. In the middle of May, a ſmall abſceſs broke at ſome diſtance from the old cicatrix, at which opening ſome matter was diſcharged, but no pieces of ligature were obſerved. Several ſmall threads were, at different times, diſcharged from the old ſore, and the ſwelling ſubſided; but the thigh ſoon ſwelled again to a greater ſize than before, attended with conſiderable pain. In the beginning of July, a piece of ligature, about one inch in length, came away, after which the ſwelling went off intirely, and he left the hoſpital the 8th of July, at which time there remained no appearance of tumor in the ham, he being in every reſpect well.

After leaving the hoſpital, the man returned to his uſual occupation of driving a hackney-coach; and being, from the nature of his employment, much expoſed to cold, in March 1787 he was ſeized with a fever of the remittent kind, which carried him off. He had not made any complaint of the limb on which the operation had been performed, from the time of his leaving the hoſpital.

He died on the 1ſt of April, 1787, fifteen months after the operation; and leave was procured, with ſome trouble and conſiderable expence, to examine the limb, ſeven days after death, at which time it was intirely free from putrefaction.

The cicatrix on the anterior part of the thigh was ſcarcely diſcernible, but the parts under it felt hard. The ham had no appearance of tumor, and was to the eye exactly like that of the other limb; there was, however, a ſolid tumor perceptible to the touch, filling up the hollow between the two angles of the thigh-bone.

The femoral artery and vein were taken out above the giving off the branch called profunda, and a little below the diviſion into the arteriæ tibiales and interoſſea. The arteries and veins that were pervious being injected, the whole was carefully diſſected.

The femoral artery was impervious from its giving off the arteria profunda as low as the part included in the ligature, and at that part there was an oſſification for about an inch and an half along the courſe of the artery, of an oval form, the rim of which was ſolid, becoming thinner towards the center, and not bony, but ligamentous. Below this part, the femoral artery was pervious down to the aneuriſmal ſac, and contained blood, but did not communicate with the ſac itſelf, having become impervious juſt at the entrance.

What remained of the aneuriſmal ſac was ſomewhat larger than an hen's egg, but more oblong, and a little flattened, extending along the artery below for ſome way; the blood preſſing with greater force in that direction, and diſtending that part ſo, as in ſome meaſure, to give the appearance of a ſeparate bag. The ſac was perfectly circumſcribed, not having the ſmalleſt remains of the lower orifice into the popliteal artery; whether this aroſe from the artery being preſſed upon by the inferior portion of the ſac, as appears to be the caſe in common, or was in conſequence of the ſac contracting after the operation, I will not pretend to determine; but it contained a ſolid coagulum of blood, which adhered to its internal ſurface. A ſection made of this coagulum, appeared to be compoſed of concentric lamellæ, uniform in colour and conſiſtence.

The popliteal artery, a little way below the aneuriſmal ſac, was joined by a ſmall branch, very much contracted, which muſt have ariſen either from the profunda, or the trunk of the femoral artery. About two inches below the ſac, the popliteal gave off, or divided into the tibiales.

The profunda was of the uſual ſize, but a good deal oſſified, for ſome length after leaving the femoral artery; the two tibials, where they go off from the popliteal, were in the ſame ſtate.

The trunk of the femoral vein, where it paſſed along the ſide of the tumor, muſt have been obliterated; for at this part it appeared to ſend off three equal-ſized branches, paſſing over different parts of the aneuriſmal ſac: theſe muſt have been dilated branches, none of them having the courſe which the trunk of the vein ſhould have purſued.

Theſe appearances throw ſome light upon the changes which took place in the limb after the operation. The ligature upon the femoral artery impeded the paſſage of the blood into the ſac ſo much, as to allow its contents to coagulate, and render the opening into it from the artery impervious. By this a ſtop was only put to the increaſe of the tumor, its reduction to the ſize met with in the dead body, muſt have been the effect of abſorption.

The conclusion to be drawn from the above account, appears a very important one, viz. that ſimply taking off the force of the circulation from the aneuriſmal artery, is ſufficient to effect a cure of the diſeaſe, or at leaſt to put a ſtop to its progreſs, and leave the parts in a ſituation from which the actions of the animal œconomy are capable of reſtoring them to a natural ſtate.

In confirmation of the cure of aneuriſm depending on taking off the force of the circulation, I ſhall mention a caſe of aneuriſm that recovered without an operation, and in which the mode of recovery depended upon the ſame principle.

The aneuriſm was in the femoral artery, and the ſwelling appeared upon the anterior part of the thigh, a little above the middle, extending upwards nearly to Poupart's ligament; an attempt was made, by compreſſing the artery above the tumor, by means of an inſtrument ſomewhat reſembling a ſteel truſs, to give the blood in the ſac a chance of coagulating,

agulating, and by that means to put a ſtop to the progreſs of the diſeaſe. But, from the pain which it occaſioned, every attempt to make a permanent compreſſion on the artery proved ineffectual. The tumor increaſed to a very conſiderable ſize, a great degree of inflammation and ſwelling took place in the ſac and common integuments, and mortification appeared to be coming on the ſkin. While in this ſtate, no pulſation could be felt in the tumor, or the artery immediately above it; ſo that the ſteps preceding mortification had taken place *, which put a ſtop to the dilatation of the ſac, and all its conſequences. From the time the pulſation in the ſac ſtopped, the inflammation and ſwelling ſubſided, although very ſlowly; and as the tumor diminiſhed it became firm and ſolid, and the patient got perfectly well.

It appears from theſe caſes, that ſurgeons have laid too much ſtreſs upon the ſuppoſed neceſſity of large collateral branches, to inſure

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* On examining the bodies of thoſe who die in conſequence of mortifications, the artery leading to the mortified part is completely ſtopped up with a firm coagulum for ſeveral inches in length; this muſt precede the mortification, and ſeems intended to prevent hæmorrhage.

From Mr. Hunter's Lectures.

ſure the ſucceſs of this operation; an opinion which muſt have ariſen from anatomical knowledge, rather than obſervations made from practice.

The ſecond time Mr. Hunter performed this operation, was upon a trooper about forty years of age.

A tourniquet having been looſely applied upon the thigh, the operation was begun by a longitudinal inciſion through the integuments, and the artery and vein were expoſed, as in the former caſe, but not taken up with a number of ligatures, for nothing appeared to have been gained by ſuch practice, and the bad effects of it were obvious in the progreſs of the cure; they were included in one ſtrong ligature, ſufficiently tight to prevent the pulſation in the ſac, without injuring the coats of the veſſels. The ends of the ligature were brought out at the wound, which was in this caſe dreſſed from the bottom. The advantages propoſed by this treatment were, to be able to ſee the progreſs of the cure, and to come readily at the artery, if any unfavourable circumſtance occurred; ſince the abſceſſes in the former caſe were ſuſpected to have ariſen from the mode of healing.

After the operation, the ſuperficial veins of the leg became exceedingly turgid and numerous, and the limb, although warm, became rather leſs ſo than the other, particularly the foot.

The next day the leg was ſwelled, and the heat 12° lower than the other; the ſecond day it exceeded the other 5°; and on the fourth day the two limbs were equally warm; the patient was free from fever.

On the fourteenth day the ligature came away, and the tournequet was looſely applied, as a precaution againſt bleeding; the ſartorius muſcle was a good deal enlarged, and covered the paſſage down to the artery, ſo as to prevent the matter from having a free diſcharge, a good deal being confined behind it, and with difficulty ſqueezed out at each dreſſing.

On the nineteenth day there was an hæmorrhage from behind the muſcle, the ſwelling of which rendered it nearly as difficult to come at the veſſel, as if the parts had healed by the firſt intention; the bleeding was ſtopped by applying preſſure, after having loſt about ten or twelve ounces.

On the twentieth there was a ſlight bleeding, which was readily ſtopped: yet five hours afterwards the femoral artery gave way, and he loſt about one pound of blood before the tournequet was applied. The artery was laid bare, and tied a little higher up, the patient being very weak and low; in this ſtate he continued till the twenty-third day without bleeding, when it bled again from a ſmall veſſel. On the twenty-ſixth a conſiderable hæmorrhage having taken place, he became faint, then delirious, had vomiting with hiccough, and died the ſame day.

Upon examining the limb, ſinuſes were diſcovered both upwards and downwards, in the direction of the artery and ſartorius muſcle, beſides ſmaller ones in different directions.

In this caſe, the bad conſequences and death of the patient, do not appear to have ariſen from the operation, but were intirely the reſult of the mode of treatment afterwards, as will appear from the following caſes.

The third patient operated upon by Mr. Hunter was a poſtillion, thirty-five years old. Compreſſion upon the femoral artery was attempted, but the pain was ſo great that it could not be continued.

In performing the operation only one ligature was uſed, and the parts healed by the firſt intention.

On the ſeventh day after the operation, the firſt dreſſings were removed, and a good deal of matter came out by the ſide of the ligature. On the fourteenth the ligature came away, and in four weeks the wound healed.

The ſac in the ham, from being chafed previous to the operation, burſt at this time, but healed up like any other ſore, and at the end of three months he was perfectly recovered.

Mr. Hunter's fourth patient was a coachman, thirty-ſix years old.

The tumor in the ham was not very large, and ſituated lower down than uſual, the whole leg being ſwelled, and the veins turgid. The pain he complained of was exceedingly violent, but being in a very bad ſtate of health, an operation was not thought adviſeable, and gentle preſſure on the tumor was attempted; but, from the pain it occaſioned, the operation was had recourſe to, as the only chance of ſaving his life, although, from the irritable ſtate in which he then was, even that ſeemed a forlorn hope.

In performing the operation, the vein was not included in the ligature, but in other reſpects it was ſimilar to the former.

Immediately after the operation the limb was benumbed, and continued ſo for ſome time, which was ſingular, as the nerve had not been included. It became, on the ſame day, four or five degrees hotter than the other leg, and continued ſo for the firſt fourteen days, when the temperature became the ſame as that of the other limb.

The ſixth day the firſt dreſſings were removed, and the ſkin was united every where except at the paſſage of the ligature. It remained in this ſtate till the twenty-firſt, when the cicatrix inflamed and ulcerated, with a ſloughy appearance, and hardneſs up the thigh.

On the twenty-ninth day the ligature came away; the ſore now put on a better appearance, ſuppuration took place where the hardneſs had been in the courſe of the artery, and the parts became ſofter, the diſcharge gradually diminiſhed, and in the ſeventh week the wound was healed.

But it did not continue ſo; for in three days an inflammation took place, and an abſceſs formed, and burſt at the cicatrix, which alſo healed up.

About the end of the tenth week he was attacked with a very ſevere remitting fever, which laſted fourteen days, and left him much reduced; but in the fourteenth week he was ſo far recovered, as to leave the hoſpital and go into the country, for the recovery of his health.

The fifth patient upon whom Mr. Hunter performed this operation was Joſeph Caſwell, aged forty-two, a man not accuſtomed to horſe exerciſe, or any mode of life which could in the leaſt aſſiſt in producing the diſeaſe. The aneuriſm was in the ham of the left leg.

In performing the operation, the artery alone was included in a ſtrong ſingle ligature, and the wound was healed by the firſt intention, leaving a paſſage for the ligature. The local inflammation was extremely ſmall, and conſequently attended with little ſympathetic fever. The ligature came away the eleventh day, and in five weeks he went into the country, able to walk with a ſtick, the wound perfectly healed.

In this caſe the heat of the two legs was caerfully examined twice a day, from the ſecond to the ninth after the operation, and the limb operated upon was uniformly colder than the other.

He came to town ſix months after the operation, and ſaid that the left leg was fully as ſtrong as the right, but, when expoſed to cold, he was more ſenſible of its effects upon that leg. About two months after the operation he had a violent pain in the upper part of the left foot, ſimilar to what is felt when a nerve is preſſed; this laſted for about ſix weeks, and afterwards went entirely off. As no nerve was included in the ligature, this affection probably aroſe from the nerve in its paſſage through the conſolidated parts being deprived of its natural freedom. There was a ſmall tumor, the remains of the aneuriſmal ſac, very diſtinctly to be felt in the ham, but without pulſation, and to the feel perfectly ſolid.

The following caſe was operated upon by Mr. Lynn, ſurgeon of the Weſtminſter Hoſpital, in the ſame manner as abovementioned; and the account of the operation is given in his own words.

"Samuel Smart, an hackney coachman, twenty-five years of age, had a popliteal aneuriſm, for which I performed the operation in the following manner. I made an inciſion down to the femoral artery, a little below the middle of the thigh, and having ſeparated the artery from the contiguous parts, I paſſed under it, by means of an eyed probe, a broad ligature, which was tied ſo as to cut off all communication with the tumor, and the lips of the wound were brought together, and retained by ſticking plaiſter, and the patient put to bed; this leg was rather colder than the other, and ordered to be fomented. The next day he was free from pain, and the limb was warmer than the other.

"On the fourth the dreſſings were removed, and the parts were found united, except at the ligature.

"On the thirteenth the ligature came away, and in the courſe of the month the whole was healed, and the patient ſoon afterwards perfectly recovered.

Wm. Lynn."

This operation of Mr. Hunter's having ſucceeded in the firſt inſtance, ſurgeons of different hoſpitals were led to adopt it, but with ſome variation, according to their own judgment, and the circumſtances of the reſpective caſes. Theſe I did not introduce in the order in which they took place; for not being performed exactly in the ſame manner, they would have interrupted the regular ſeries of thoſe caſes above related. But I ſhall now give them either in the words of the ſurgeons who performed the operations, or as correctly as I can from having myſelf been an eye-witneſs; and although they were not attended with ſucceſs, that circumſtance will not be found to affect the propriety of the mode of performing this operation which has been recommended above.

In a caſe of aneuriſm of the femoral artery, the operation was performed by Mr. Birch, ſurgeon to St. Thomas's Hoſpital, who relates the hiſtory as follows:

" John Lewis, a negro, aged forty-three, received a blow on the anterior part of the right thigh; about a month after he perceived a ſmall tumor, which gradually increaſed, and his own expreſſion was, that he could feel it thump, thump.

As the tumor enlarged, he came to London for advice, applied at St. Thomas's Hoſpital, on Thurſday, the 26th of October, and was directly admitted. On examination I found a large tumor, extending within two inches of Poupart's ligature upwards, and occupying two-thirds of the thigh; a pulſation could be felt, and there was no doubt of the diſeaſe being an aneuriſm of the femoral artery.

I directed ſeven ounces of blood to be taken from the arm, and an opiate to be given at night; the patient reſted well, and the next day a conſultation was held, in which it was propoſed to perform an operation, and endeavour to paſs a ligature round the femoral artery, giving the patient the chance of nouriſhing the limb by the arteria profunda, and other anaſtomiſing veſſels.

On Friday, the 3d of November, it was determined to perform the operation; Mr. Cline undertook to compreſs the artery as it paſſes through Poupart's ligament, which he eaſily effected with a hard compreſs, in the ſhape of a T, with a broad baſis.

It was agreed, previous to the operation, that an inciſion ſhould be carried in a ſemilunar form round the upper part of the aneuriſmal ſac, in order to make room for the longitudinal inciſion neceſſary to diſſect down to the artery; this was accordingly done, and the integuments raiſed, ſo as to make room to feel for the pulſation of the artery; ſome portion of cellular membrane and lymphatic glands were neceſſarily diſſected and removed: with my fingers I then ſeparated the muſcular fibres, and tore away the connecting parts till the artery could be plainly felt in pulſation; it was then neceſſary to divide a part of the faſcia covering the artery, which was done by carrying the back of the knife on Mr. Cline's nail, while his finger preſſed upon the naked artery; after which the finger and thumb could ſurround and compreſs the veſſel; an eye probe, armed with a ſtrong flat ligature, was then puſhed through the cellular membrane, and carried under the artery. This being effected, we had ſuch command of the veſſel as to be able to ſtrip it down, and paſs another ligature ſomewhat lower. This laſt ligature was then tied, the firſt being left looſe, to ſecure us againſt accident.

The threads being ſeparated and ſecured, the wound was lightly dreſſed, the tumor left in its natural ſituation, and the patient put to bed, with the loſs of only four or five ounces of blood during the operation. No pulſation could be perceived in the tumor after the ligature was tied.

On Saturday, November the 4th, he had ſlept well, was eaſy, and there was ſufficient warmth in the extremity to aſſure me of ſome circulation. On the 5th the diſcharge from the wounded lymphatics was ſo abundant, as to make it neceſſary to remove the ſuperficial dreſſings; the tumor was rather ſofter to the touch, and the ſkin about the apex of it began to ſhrivel.

The diſcharge of lymph continued till the 9th, and then the wound began to digeſt, affording, however, a very ſmall quantity of pus. The tumor grew thinner at one point, and ſeemed as if diſpoſed to ulcerate the integuments. This day I paſſed a bleeding ligature round the leg, juſt below the knee, and the veins tumified ſufficiently to have bled freely if they had been punctured.

10th. He was feveriſh in the evening.

11th. He had ſtools from ſome laxatives I had directed, and was better.

12th. The tumor was very thin at one part, and a fluctuation evidently to be felt.

The limb was warm and moveable, but the patient was feveriſh, and delirious at night; a decoction of bark, with a ſedative bolus, was directed for him, but he would not take them.

13th. The wound looked florid, and afforded good pus, the patient was feveriſh and delirious, the tumor was threatening to burſt. This day he took his medicines.

14th. He became ſenſible, but was languid and hot; the tumor burſt, and diſcharged ſerum and grumous blood; he fainted; the dreſſings were not diſturbed; he ſlept compoſedly; fainted again about ſix o'clock in the evening, and expired. I ſaw him at ſeven, when the limb was ſtill warm; I removed the dreſſings, and found a ſmall ſtream of freſh arterial blood which had iſſued from the wound.

It appears probable that if the patient had applied for relief before the tumor was ſo much enlarged, the operation might have ſucceeded, as we ſhould then have been able to have tied the ſound artery ſo much lower down.

J. Birch.

The body was examined the morning after the patient's death by Mr. Cline.

The integuments on the middle of the tumor were mortified. The blood contained in the tumor was very putrid, and the greater part of it fluid, it appearing to have been diſſolved by putrefaction.

Water injected by the external iliac artery, eſcaped freely from the wound at the ligature where the artery was open, and appeared to have ulcerated at that part.

In laying open the artery from the ligature to the heart, its internal ſurface appeared of a bright red. This appearance leſſened at the curvature of the aorta, yet it was very evident in its ſemilunar valves.

The arteria profunda, which paſſed off from the femoral artery rather leſs than half an inch above the ligature, was alſo inflamed within. There were near two inches of the femoral artery between the ligature and the aneuryſmal ſac, the internal ſurface of which was of the uſual white colour; from this a membranous-like ſubſtance could be peeled off that ſeemed to reſemble coagulable lymph.

The opening where the artery paſſed out of the aneuryſmal ſac was near three inches below the part where it entered. In opening this part of the artery from the ſac to the ham it appeared quite ſound, and of its natural colour. H. Cline.

This mode of operating was adopted by Mr. Pott in a caſe of popliteal aneuryſm in St. Bartholomew's Hoſpital; which operation having been the object of medical attention for ſome time, I ſhall give a brief account of it, as I was preſent at the time it was performed.

Mr. Pott began the operation by making an inciſion, about five inches in length, upon the poſterior part of the thigh, through the common integuments, a little higher than the tumor, and in the direction of the thigh, between the two ham-ſtrings; he then diſſected down to the veſſels at the upper end of the inciſion, which being there deep ſeated, proved both tedious and difficult. Having come to the veſſels, a double ligature was paſſed, and the two portions tied ſeparately, at nearly half an inch diſtance. The depth of the inciſion made it difficult for any but the operator, and thoſe immediately aſſiſting him, to ſee what was included in the ligature, and at the time the popliteal artery was ſuppoſed to be ſecured by it. The wound was dreſſed up in the common way.

The ſecond day after the operation, a pulſation was felt in the tumor, which afterwards enlarged ſo much that Mr. Pott amputated the limb.

It is ſaid, that the aneuryſm appeared, upon an examination of the limb, to have been in an anaſtomiſing branch, not in the trunk of the artery.

The following remarks upon this operation will tend in ſome meaſure to illuſtrate the method recommended and practiſed by Mr. Hunter.

The mode of taking up the artery in the ham muſt be always unfavourable to the future ſucceſs of the operation, if either the artery itſelf ſhould be diſeaſed, or if the tumor, by being ſo contiguous to the violence done in the operation, ſhould be affected by the conſequent inflammation; which ſeems to have been the caſe in Mr. Pott's operation, as I underſtand two abſceſſes were formed cloſe to the ſides of the ſac.

Had the aneuryſm been ſituated in an anaſtomiſing branch of the popliteal artery, given off below the ligature, there ſhould have been no pulſation afterwards in the tumor; and were it in a branch going off above the ligature, the pulſation in the tumor ſhould have continued immediately after the operation, and ſhould have been increaſed by it; neither of which effects appear really to have taken place, which throws a doubt upon the ſituation of the aneuryſm, if the popliteal artery was rendered impervious by the compreſſion of the ligature.

Mr. Cline, Surgeon to St. Thomas's Hoſpital, performed the operation for the popliteal aneuryſm in the following manner, at which I was preſent; and although not exactly as recommended by Mr. Hunter, it was very nearly ſo. The particulars of the caſe I have not received from Mr. Cline, but have taken them from my own obſervation, and the information of gentlemen who attended the patient, and were preſent at the examination after death.

The patient was a ſailor, who came into St. Thomas's Hoſpital to undergo the operation for the popliteal aneuryſm.

Mr. Cline made a longitudinal inciſion on the anterior part of the thigh, and having laid bare the artery, paſſed, by means of a tin inſtrument, a double tape, about one inch broad, behind the artery, the two pieces of tape lying one over the other; the piece of tin which conducted the tape was cut off, and a cork, nearly an inch long, was laid upon the artery, and confined to its ſituation by means of the upper tape, producing in this way a ſufficient preſſure upon the veſſel included between the ligature and cork to ſtop the circulation, and conſequently the pulſation in the tumor in the ham; the other portion of tape was left looſe. The intention of ſecuring the artery in this way, was to compreſs the ſides of the veſſel together, and produce an union without ulceration.

The patient went on very well, and the ninth day the tapes were removed, and every thing ſeemed to be going on very favourably, when the patient was attacked by a fever, (which was ſuppoſed to be caught from another patient in the ſame ward) of which he died.

Upon examining the ſtate of the limb after death, it was found that ulceration had taken place through the whole extent of the artery included in the tape; and ſinuſes were formed both upwards and downwards, in the courſe of the thigh, to ſome diſtance.

I cannot conclude this paper without obſerving, that it is ſeldom in giving an account of a new operation, we are able to collect materials ſufficient to render it ſo ſatisfactory as the preſent, having in our poſſeſſion not only the ſucceſsful and unſucceſsful caſes, but alſo an account of the appearances after death, under both circumſtances, ſo that the cauſes of failure are rendered evident in thoſe inſtances in which it did not ſucceed, and the means that are likely to inſure future ſucceſs are clearly pointed out.

The operation is in itſelf ſimple; it requires but a ſhort time in the performance, and produces little, if any, affection of the conſtitution; but its advantages are more clearly ſeen by contraſting it with the common mode of operating for the popliteal aneuryſm. This is by expoſing the ſac in the ham through its whole extent, laying it open, ſcooping out the blood, ſearching for the two orifices leading into it, and taking up the artery with a ligature both above and below the ſac. When this operation is over, there remains a large deep-ſeated ſore, compoſed of parts not perfectly in a natural ſtate, and in a moſt diſadvantageous ſituation; which ſore is to ſuppurate, granulate, and heal; a proceſs that is not ſoon performed, and which muſt leave a ſtiff knee for ſome time afterwards. Yet this is conſidering the operation in the moſt favourable view, ſince there is always a riſk, from the artery being diſeaſed ſo cloſe to the ſac, of the patient dying from a ſecondary bleeding; and when that does not happen, there is ſtill ſome danger of not being able to ſupport the conſtitution during the healing of a large ſore, under circumſtances ſo very unfavourable.

It is in compariſon with this operation, the only one before in uſe, that the preſent improvement is to be conſidered, and it is in this view that I have thought it deſerving the attention of the Society.

I cannot cloſe this account without inſerting the following caſe, in addition to thoſe already ſtated, in favour of this operation. I have received it juſt time enough to give it a place, as the paper was in the preſs before it came to hand, and feel myſelf obliged to Mr. Earle for his readineſs in communicating it.

Hanover-Square, *March* 10, 1792.

SIR,

AT your requeſt I ſend you ſome account of the following caſe, and am

Your moſt obedient Servant,

JAMES EARLE.

JOHN SMITH, about fifty years of age, was received into St. Bartholomew's Hoſpital on account of a fever. After having been under the care of the phyſician ſome time, he complained of a ſwelling and pain in his left leg, for which I was deſired to viſit him. He ſaid, about ſix months before, he had fallen from a ſcaffold; that his leg was caught between the rounds of a ladder, which broke his fall; that he felt immediately pain in the upper part of his leg; ſoon after it began to ſwell, and had gradually increaſed to its preſent ſize. On examination there appeared a large hard ſwelling under the heads of the gaſtrocnemii muſcles, reaching up to the bend of the leg. A pulſation was plainly to be felt in it, and there was no doubt of its being an aneuryſm. It was now increaſing very faſt in ſize; the tumor, by its preſſure, cauſed exquiſite pain, all the lower part of the leg was loaded with œdematous ſwelling, and it became abſolutely neceſſary to perform ſome operation to prevent a mortification taking place.

Having noticed with much ſatisfaction the ſucceſs which attended Mr. Hunter's method of tying the artery in the thigh, in a ſimilar caſe, I decided in favour of that operation; but as, in the preſent inſtance, the artery appeared to be in its natural and perfect ſtate in the ham, and in its whole courſe, till it reached the dilatation below the knee, I preferred taking it up in that part, rather than to tie it in the middle of the thigh, under the ſartorius muſcle; though it lies there more ſuperficial, and more eaſily to be got at; becauſe I thought the chance of the circulation being carried on was equal, if not greater, and if it ſhould fail, and ſymptoms ſhould occur to create a ſuſpicion of an impending mortification, there might be an opportunity of removing the limb above the ligature, which would be impracticable if the artery was tied in the middle of the thigh.

Jan. 28, 1792. The patient being laid on his face, and the tourniquet looſely applied, I made an inciſion about five inches long, in the direction of the artery, within thoſe tendons which compoſe the inner ham-ſtring. I then gradually ſeparated the cellular ſubſtance; in doing this the nerve was expoſed, which ran in its uſual courſe, external to the artery, and much more ſuperficial. In finding the artery, ſome difficulty occurred on account of the tumefaction of all the parts, affected by their vicinity to the aneuryſm, and from the imperceptibility of pulſation in the artery till it was actually laid bare: however, having diſcovered it in its uſual ſituation near the bone, and in its natural undilated ſtate, I paſſed a ligature round it, about two inches above the tumor. I now again examined, and being convinced that the artery was included alone in the ligature, I gradually made it tight, till I felt a pulſation above it, and none below, when I deſiſted, concluding that any preſſure beyond this degree would be uſeleſs and dangerous.

I will juſt obſerve here, that I found the common aneuryſmal needle with a handle very inconvenient, and would recommend in this caſe, where the artery lies ſo deep, a

blunt

blunt ſemicircular needle, with the eye about half an inch from the end, without any handle.

The wound was cloſed in the uſual manner, and the edges brought together by ſutures. On the following day the man was free from pain, the tumor much leſs tenſe or hard, and the whole leg greatly unloaded.—No perceptible alteration in the heat of the limb could be remarked;—when the current of blood was obſtructed in the ſuperficial veins by preſſure, on its removal they immediately again became turgid; and in ſhort every appearance indicated a continuance of perfect circulation.

On the 15th day ſucceeding the operation the ligature came away, the limb was ſoft and unloaded, and the inciſion nearly healed; at the diſtance of ſix weeks there remains a ſmall tumor, with ſome perceptible fluctuation. The patient cannot perfectly extend his leg, but is able to walk with the aſſiſtance of crutches.

X. *A Caſe of Paralyſis of the Muſcles of Deglutition; cured by an artificial Mode of conveying Food and Medicines into the Stomach.* *By* JOHN HUNTER, Eſq. F. R. S. *Surgeon Extraordinary to the King, and Surgeon General to the Army.* *Read September* 21, 1790.

DISEASES which are not mortal in themſelves, may often, from their ſecondary effects, become the cauſe of death; but if theſe ſecondary effects are removed, the diſeaſe frequently admits of a cure, or even ceaſes of itſelf. Thus diſeaſes, when they attack the vital organs, may, from the nature of the parts alone, prove fatal; and this will be ſooner or later, according to the particular functions of the parts diſeaſed.

Obſtructions to breathing, to the paſſing of the urine, to the act of ſwallowing, or the diſcharge of the fæces, will all terminate in death, if continued for a certain length of time, whatever be the nature of the diſeaſe.

Difficulty in ſwallowing, the ſubject of the preſent paper, may ariſe from a variety of cauſes, ſince an obſtruction in any part of the canal leading from the mouth to the ſtomach, will produce that complaint.

The ſwallowing our food is a complicated operation, and includes two different actions; the firſt of theſe conveys it into the œſophagus; the ſecond carries it along that canal into the ſtomach; and the parts which perform theſe two actions are in themſelves diſtinct; ſo that one may be obſtructed, or diſeaſed, while the other remains perfectly ſound.

The firſt action is often impeded, or wholly prevented by inflammation, but this is ſeldom of long continuance, and the cure is probably promoted by the patient's being obliged to abſtain from food. It is alſo impeded by ſwellings in the neighbouring glands, but this will commonly be in a leſs degree; and where it threatens ſuffocation, or a total prevention of deglutition, the patient can often be relieved by an operation.

The ſecond action is ſometimes obſtructed by ulcers in the œſophagus, which have a cancerous appearance; ſometimes ſtrictures in that canal, which are of a permanent nature; alſo ſpaſmodic contractions, which are commonly confounded with ſtrictures, but may, I believe, be readily diſtinguiſhed both from ſtrictures and ulcerations.

Ulcers and ſtrictures, from being permanent diſeaſes, in the end deſtroy the patient; but ſpaſmodic contractions admit of being cured. I have known a caſe of this kind get well in a fortnight from the uſe of electricity, which had not been at all relieved by mercury, although the mouth was affected by it for a month.

There is another cauſe of difficulty in ſwallowing, which is the reverſe of contraction, viz. paralyſis; whether the muſcular coats of the œſophagus are ever affected by it, I cannot determine; but the muſcles of the pharynx have become paralytic, and the patient has died of hunger.

As this diſeaſe is only rendered dangerous from the want of a ſubſtitute for deglutition, it becomes our duty to adopt ſome artificial mode of conveying food into the ſtomach, by which the patient may be kept alive while the diſeaſe continues, and ſuch medicines may be adminiſtered as are thought conducive to the cure.

A ſucceſsful inſtance of this practice is given in the following caſe of well marked paralyſis in the muſcles of the pharynx; the relation of which is given by the patient.

John S——l, about fifty years of age, became hypochondriac, and attacked by what are called nervous diſorders, in conſequence of anxiety of mind, brought on by various diſtreſſes. In the beginning of his complaints, he felt ſomething crack within his head, and from that period his ſenſations became very acute, his paſſions, ſympathies, and averſions, exceedingly ſtrong.

December 28, 1786, while under the influence of an uncommon degree of hypochondriaſis, about one o'clock in the morning, he brought up a glutinous ſubſtance from his ſtomach, like the white of an egg, which ſeemed to relieve him, but did not remove an averſion which he had taken to his relations, his children, and even to life itſelf, of which he was tired.

He afterwards continued affected with low ſpirits and pains in the head, became reſtleſs, and turned his mind to religious ſubjects; as theſe affections either increaſed or diminiſhed, he had frequent and ſevere fits of crying.

About the 21ſt of February, 1790, he had a violent cold in his head, with a conſiderable running from the noſe; and on the 7th of March, between one and two in the morning, awoke with a ſenſe of choaking, and felt ſoon after a numbneſs in the whole of his right ſide, together with a paralyſis of the muſcles of deglutition, which deprived him of the power of ſwallowing.

By the advice of a phyſician he was cupped, bliſtered round the throat, and electrified; he was, in the mean time, ſupported by nouriſhing clyſters. March the 9th, finding no benefit from this plan, he waited upon Mr. Hunter, with a letter from Mr. Cumming, watchmaker, accompanied by Mr. Duncan the ſurgeon.

Mr. Hunter conſidering the ſupport of the patient, and ſome mode of adminiſtering medicine, to be the firſt object, propoſed that a hollow flexible tube ſhould be paſſed down into the ſtomach, through which he might receive nouriſhment and medicines; and mentioned his having an inſtrument of that kind, made of ſpiral wire, covered with gut for the purpoſe of injecting liquids into the ſtomachs of animals, which might be applied to the preſent caſe; but that Mr. Cumming was better able to aſſiſt them in getting a tube that would anſwer that purpoſe. He alſo recommended, when the tube was made, to inject jellies, eggs beat up with a little water, ſugar, and milk, or wine, by way of food, and that the medicines might be mixed with it.

In compliance with theſe directions they contrived a tube, and injected into the ſtomach ſome of the abovementioned ſubſtances twice a day; three drams of valerian were mixed with this food, increaſing the doſe to ſix drams. Laudanum was given in the doſe of forty drops, by clyſter, and afterwards thirty drops by the mouth; but both doſes affected the head, without procuring reſt. The valerian was continued till the 26th of March, at which time he had taken four ounces, and had evidently recovered a degree of ſenſation in his throat. Mr. Hunter was again conſulted, and ordered two ſcruples of flour of muſtard, and one dram of *tincture* of valerian, twice a day; under this treatment the power of ſwallowing gradually returned, and on the 29th the uſe of the tube was no longer neceſſary. The natural ſenſation of the throat and right arm had not returned in the month of April.

The inſtrument made uſe of was a freſh eel-ſkin, of rather a ſmall ſize, drawn over a probang, and tied up at the end where it covered the ſponge, and tied again cloſe to the ſponge where it is faſtened to the whalebone, and a ſmall longitudinal ſlit was made into it juſt above this upper ligature. To the other end of the eel-ſkin was fixed a bladder and wooden pipe, ſimilar to what is uſed in giving a clyſter, only the pipe large enough to let the end of the probang paſs into the bladder without filling up the paſſage. The probang, thus covered, was introduced into the ſtomach, and the food and medicines were put into the bladder, and ſqueezed down through the eel-ſkin.

The inſtrument did not produce irritation in the fauces or œſophagus; in ſuch caſes, the parts loſing their natural ſenſibility, allow greater liberties to be taken with them. An eel-ſkin ſeems very well adapted for this purpoſe, being ſmooth, pliable, and readily paſſed into the ſtomach; but as caſes of this kind may occur where eels cannot be procured, a portion of the gut of any ſmall animal, as a cat or lamb, will make a very good ſubſtitute.

XI. *Of a remarkable Deviation from the natural Structure in the urinary Bladder and Organs of Generation of a Male. By* MATTHEW BAILLIE, M. D. F. R. S. *and Phyſician to St. George's Hoſpital.*

Read January 18, 1790.

THE ſubject of the preſent paper does not ſeem to throw any conſiderable light either on the phyſiology or pathology of the human body, yet it is not altogether unimportant. It exhibits a view of a ſingular variety in a very eſſential part of the ſtructure of the human body, has been rarely obſerved, and, I believe, has not as yet been fully deſcribed *.

The perſon in whom this variety was found was a man of about forty years of age, of a ſhort ſtature, of a robuſt habit, and of a very diſſolute life. Having occaſionally got a good deal of money, by exhibiting himſelf to public curioſity, and having acquired habits of idleneſs, he very readily fell into drunkenneſs, the moſt prevailing vice among the lower claſs of people. He was at length very rarely ſober, and in one of his fits of inebriety he died. Through the kind offices of one of my medical friends, he was brought to Windmill-ſtreet for examination.

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* There is a ſhort hiſtory of a ſimilar monſtroſity, by James Mowat, ſurgeon at Langholm, in which many particulars have been omitted. Vid. *Edinb. Eſſays*, Vol. III. article 14.

DESCRIPTION. *External View.*

In the external view of the abdomen was ſeen, immediately above where the ſymphyſis pubis is commonly placed, a vaſcular pulpy maſs, about two inches in tranſverſe diameter, and an inch and an half from its upper to its lower edge *. This maſs was ſeated in a deep bed, ſcooped out, as it were, from the lower part of the ſkin and muſcles of the abdomen. When this maſs was examined a little more particularly, it ſeemed to reſemble ſomewhat the poſterior ſurface of the tongue, but without any follicular appearance. In three places it projected into tubercles of no very conſiderable ſize, and upon two of theſe the ureters opened. At the upper edge of this maſs the ſkin was in one part finer than is natural, and a little puckered. This was all the appearance of navel in this ſubject, for there was none in the uſual place. At the lower edge of this maſs, and about the middle of that edge, were ſituated two diſtinct rounded orifices, which were found to be the openings of the vaſa deferentia and veſiculæ ſeminales. Between theſe two orifices, and a little above their level, was a conſiderable canal, large enough to admit a gooſe quill, and leading to the ſubſtance of the proſtate gland.

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* This pulpy maſs was covered with a thick layer of viſcid mucus in the living body, in order to defend it from the ſtimulus of the ſalts contained in the urine.

About half an inch under the vaſcular maſs I have deſcribed, the penis was ſeen, of the common ſize with reſpect to tranſverſe diameter, and about a little more than an inch in length. The whole of the upper ſurface of the penis was ſomewhat hollowed, and had a finer ſkin than the common covering of that part. Near its extremity, the canal correſponding to the urethra opened by a pretty large oval orifice. The glans was of the common texture, but ſomewhat different in its ſhape, being hollowed upon the upper ſurface. It was covered by a thicker cuticle than uſual, from its being bare and expoſed to friction. Under the glands there was a looſe projection of ſkin, correſponding to the prepuce. It was connected to the glans by a chord like the frænum, but was neither ſufficiently pliable nor large to cover the glans, as in ordinary caſes.

There were two conſiderable oblong ſwellings at the groin, covered with hair. Upon the inſide the ſwellings were formed of fat, and had paſſing through them the ſpermatic chords. They terminated below in a ſort of corrugated tuberculated ſkin, forming the ſcrotum. The cellular membrane on the inſide of it, inſtead of being ſpungy as uſual, was of a very firm compact texture. By this membranous texture the teſticles adhered very firmly to the inſide of the ſkin of the ſcrotum, and the ſcrotum was divided into two lateral cavities, by a ſtronger ſeptum than I ever recollect to have ſeen.

INTERNAL VIEW.

Upon the inſide, correſponding to the ſituation of the vaſcular ſpungy maſs, there was a ſort of projection, not unlike the poſterior outer view of the urinary bladder. It conſiſted of an arrangement of muſcular fibres, ſimilar to thoſe in the bladder, but ſomewhat ſtronger, and had opening through it the two ureters. Theſe ureters were of a larger than the ordinary ſize, and had probably been enlarged artificially.

This perſon had a cuſtom of thruſting a knitting-needle up each ureter when he exhibited himſelf to gratify curioſity, which might, perhaps, encreaſe a little the ſize of the ureters near their external openings.

Above this appearance of bladder, there was a conſiderable ſpace between the muſculi recti abdominis occupied by tendon and the peritonæum. In the middle of this ſpace paſſed the umbilical vein, degenerated into a ſort of ligament, ſo as to reach the upper edge of what correſponded to the bladder, where it was loſt behind the integuments. The umbilical arteries climbed up each ſide of what correſponded to the bladder, and loſt themſelves behind the integuments at the ſame place with the vein. This place correſponded to the fine puckered ſkin above the vaſcular maſs mentioned in the external deſcription.

Upon each ſide of the ſubſtance correſponding to the bladder, the recti muſcles paſſed, to terminate in the oſſa pubis, at nearly four inches diſtance from each other.

Between the ſubſtance correſponding to the bladder and the rectum, were ſituated the proſtate gland, the veſiculæ ſeminales, and the extremities of the vaſa deferentia.

The proſtate gland was ſcarcely one half of its uſual ſize, and had an opening externally under the muſcular maſs by a large canal, as formerly mentioned. The veſiculæ ſeminales were ſcarcely more than a third of the ordinary ſize; the extremities of the vaſa deferentia were enlarged as in the natural ſtructure, and opened by two rounded orifices externally under the vaſcular maſs.

The rectum was, in its appearance, perfectly natural, nor was there any reaſon to expect it ſhould be otherwiſe.

The penis aroſe by two crura from the aſcending rami of the oſſa iſchii, being nearly of the uſual ſize.

The crura aſcended for more than two inches before they united, and between them was ſituated the corpus ſpongioſum urethræ. In that body was a canal, correſponding to the urethra, having the ſame vaſcular lining, and the openings of lacunæ. This canal communicated with a conſiderable cavity, ſituated at the bulb of the urethra, and having the ſame lining of membrane with the urethra itſelf. In this cavity could only be ſeen the orifices of the lacunæ, and poſteriorily it terminated in a cul de ſac.

The erectores penis and muſcles correſponding to the acceleratores urinæ, had very nearly the natural appearance, but the tranſverſi perinæi were ſeven or eight times larger than uſual. The muſcles about the anus were exactly natural.

The teſticles were of the common ſize, but the tunica vaginalis was almoſt obliterated on each ſide, probably from inflammation. Their texture was perfectly natural; they were ſupplied in the common way with blood-veſſels, and the vaſa deferentia and epidydimis ſeemed of the ordinary ſtructure. The vaſa deferentia and a portion of the epidydimis were injected with quickſilver, to ſhew that the canals were pervious. On each ſide the cremaſter muſcle was ſtronger than I almoſt recollect to have ſeen it in any body. There was alſo a hernia upon the right ſide, in which a portion of the omentum was lodged.

In the anterior part of the pelvis, where the ſymphyſis pubis is uſually ſituated, there was a deficiency of bone for nearly four inches. This was filled up by a broad ligamentous membrane, becoming a ſupport to the viſcera of the abdomen, and keeping ſome of the parts which I have mentioned in a fixed ſituation. The two acetabula were alſo removed at ſomewhat a greater diſtance from each other than in an ordinary male pelvis.

In every other part of the body of this perſon, the ſtructure was perfectly natural.

Such are the particulars of this very uncommon monſtroſity, and they afford very little room for reflection.

It appears clearly, from the account we have given, that this perſon was in every reſpect a male, and had no mixture whatever of the other ſex. The teſticles and vaſa deferentia were natural, ſo that the ſecretion of the ſemen could take place as in ordinary caſes.

This ſtate of the teſticles was probably accompanied by the venereal appetite as in ordinary men, but ſtill he may have been ſaid to have been impotent.

The vaſa deferentia terminated by two orifices at the lower part of the abdomen, and there was no canal by which the ſemen could be ejected with force ſufficient for impregnation.

The proſtate gland too and veſiculæ ſeminales were in this perſon of a very diminutive ſize, ſo that their function, whatever it may be as ſubſervient to impregnation, was probably imperfect.

The openings of the vaſa deferentia did not bear the ſame relative ſituation to the penis as in the ordinary ſtructure; yet I have no doubt that titulating the glans, ſo as to produce erection, promoted the ſecretion of the ſemen as in common men. We are led to this opinion from conſidering the general ſtructure of his organs, and it is in ſome meaſure confirmed by the imperfect knowledge we have of his life. There was a woman who lived with him as his wife or miſtreſs, and, from his very impudent behaviour in the company of women, he would appear to have been ſufficiently ſalacious.

The want of a bladder in this perſon ſhews ſimply, that it is not a part eſſentially neceſſary to the body, but is only a reſervoir to contain a certain quantity of urine, for the greater convenience of the animal. This might be readily inferred a priori from reaſoning, without a ſingle inſtance of it having occurred; but it is certainly known from the claſſes of animals in whom no bladder is naturally found.

The deficiency of the pelvis at the anterior part, and the diſtance between the two acetabula being a little greater than uſual, had ſome effect upon his manner of walking. His ſtep was leſs firm, the lateral motion of his body greater, and his progreſs conſequently ſlower, than in common men *.

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* Since this paper was written, I have met with a monſtroſity of the ſame kind, in a young man about twenty, who is a native of Cologne. In the external appearance of the abdomen there is no difference between this caſe and that which I have deſcribed, except that in the former the vaſcular pulpy maſs was more protuberant than in the latter, and the opening of the ureters more dependent. The conjecture which I have formed about the venereal appetite is confirmed by what I have learned from this perſon. His penis is often erected at the view of females, but, from the ſtructure of the parts, he is incapable of producing impregnation.—A deſcription of the external appearance of the parts in this perſon has, I underſtand, been given by profeſſor Bonn of Amſterdam.

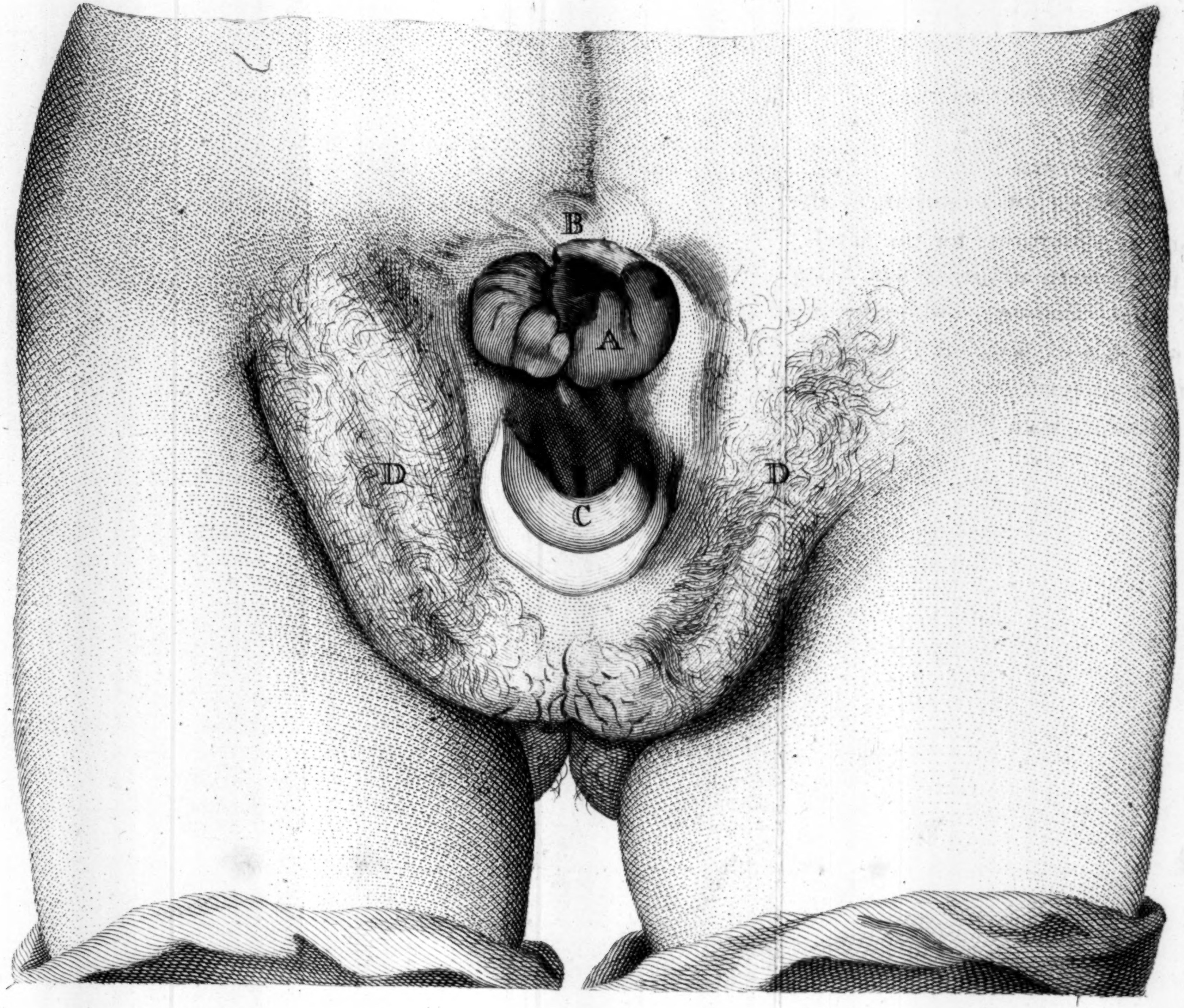
B
A
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PLATE VI.

A.	Pulpy vaſcular maſs.
B.	The appearance of a cicatrix in the ſkin, as a ſubſtitute for the navel.
C.	The penis, with an orifice ſeen upon its upper ſurface; and under it a thick fold of ſkin, as a ſubſtitute for the prepuce.
D. D.	The ſcrotum, with hairs upon it.

PLATE VII.

FIG. I.

A A.	Portions of the muſculi recti abdominis, ſeparated at a conſiderable diſtance from each other.
B.	The umbilical vein ſeen over two briſtles, ſhrunk into a ſolid maſs.
C.	The poſterior view of what correſponded to the bladder, with the two ureters ſeen entering into it. The ureters contain bougies.
D.	A ſmall imperfect proſtate gland.
E E.	Very ſmall imperfect veſiculæ ſeminales.
F F.	The vaſa deferentia.
G.	The rectum, with its internal vaſcular membrane ſeen.
H H.	The origins of the corpora cavernoſa penis ſeparated at a great diſtance from each other.

Fig. 1.

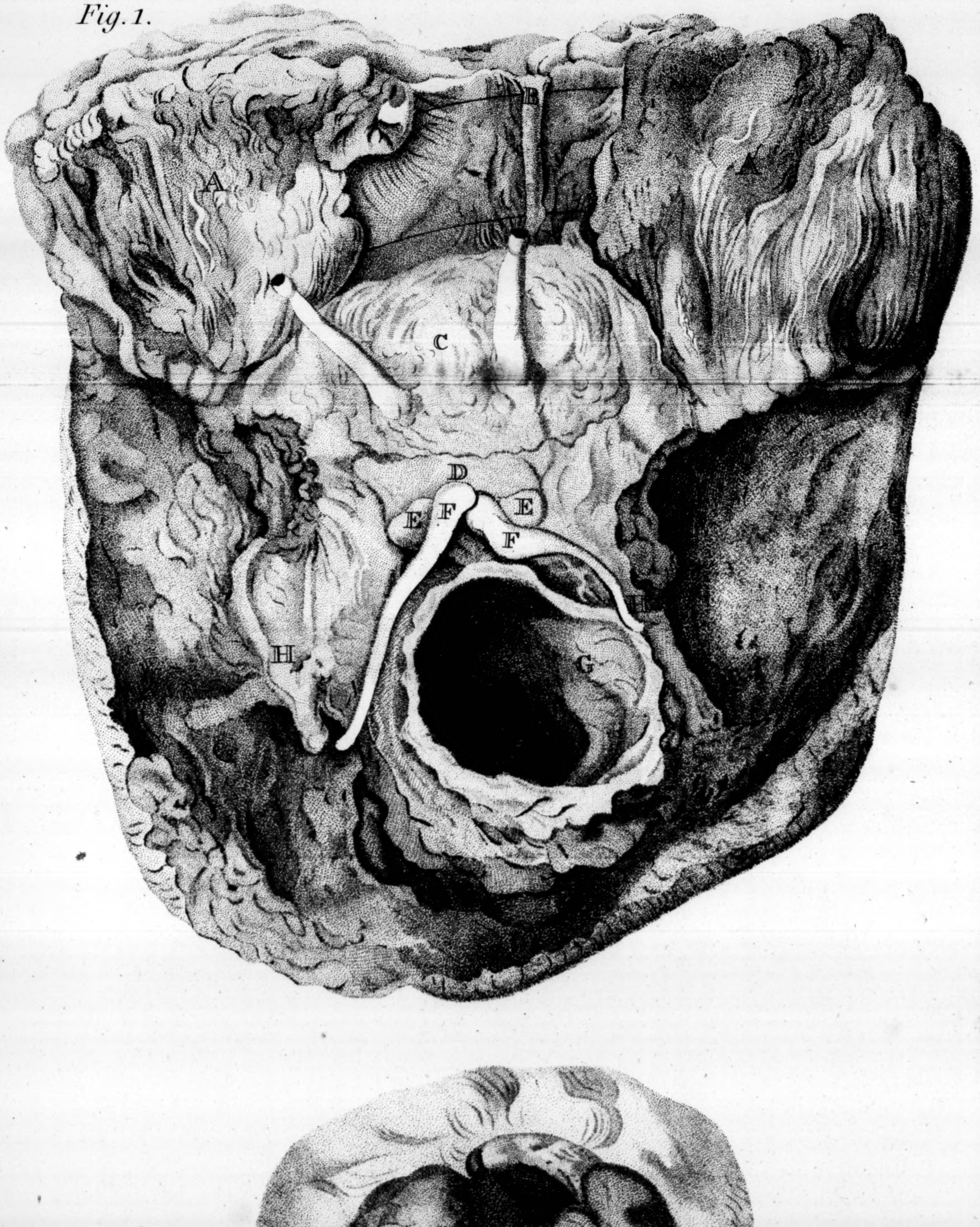

Fig. 2.

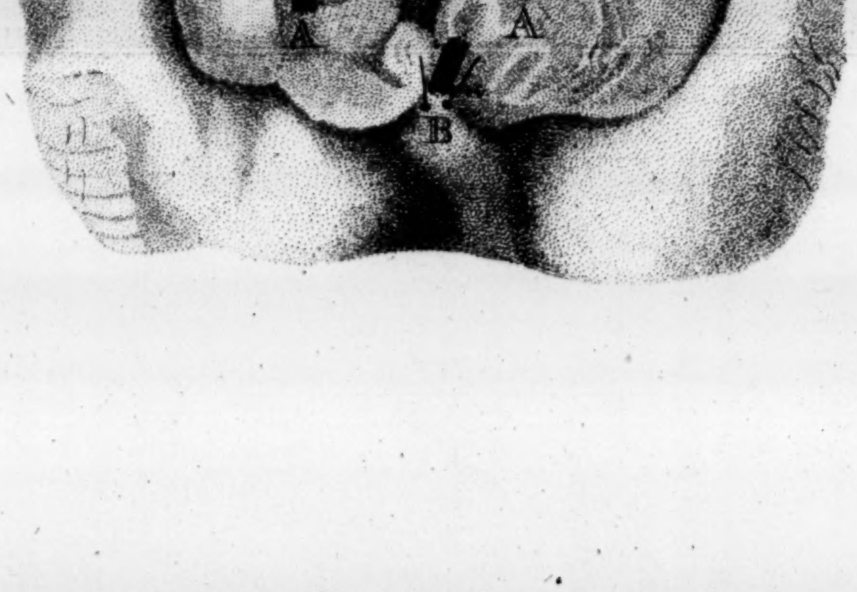

FIG. II.

A A. Two bougies in the external openings of the two ureters upon the pulpy maſs.

B. Two briſtles in the openings of the veſiculæ ſeminales, and a bougie in an opening which led to the proſtate gland.

XII. *A Caſe of* EMPHYSEMA, *not proceeding from local Injury.* *By* MATTHEW BAILLIE, M.D. F.R.S. *Phyſician to St. George's Hoſpital.* *Read July* 19, 1791.

I HAD lately an opportunity of obſerving a very uncommon appearance of diſeaſe in the living body, as well as of examining its extent after death.

Margaret Buck, aged about ten years, was admitted nearly three months ago a patient into St. George's Hoſpital, with anaſarca and aſcites. Two days before her death I was deſired by the apothecary of the hoſpital to ſee her, as Dr. Ford, the phyſician, who had for ſome time attended her, could not come on that day to the hoſpital. When I ſaw her lying in bed, her appearance differed in nothing from that of a perſon highly anaſarcous, nor ſhould I have at all ſuſpected that there was any peculiarity in her complaint. Upon attempting to feel her pulſe, however, I was ſurprized by the crackling of air under my fingers, and the ſkin was conſiderably elevated, ſo that the pulſe by that means was indiſtinctly felt. I then preſſed upon the ſkin of the back, breaſt, belly, &c. and there was the ſame crackling of air. Air was alſo felt in the cellular membrane of the inſide of the thighs. There was at the ſame time a conſiderable quantity of water accumulated in the cellular membrane of the legs and face, as could be readily diſtinguiſhed by the doughy feeling upon preſſure. The girl herſelf lay in a ſort of ſtupid ſtate, expreſſing, however, a ſenſe of conſiderable pain, and having a good deal of difficulty in breathing.

After having examined the patient I ſent for Mr. Walker, one of the ſurgeons of the hoſpital, who was at that time in the houſe, that he might have an opportunity of ſeeing this very curious appearance of diſeaſe, and deſired that ſmall openings might be made through the ſkin for emptying the air. The patient died on the next day, and I took an early opportunity of examining the full extent of the diſeaſe by diſſection.

Air was found diffuſed through the cellular membrane of the trunk, arms, thighs, &c. as formerly related. Upon making an opening into the cavity of the abdomen, the ſtomach was found diſtended, almoſt as far as it could ſtretch, with air, and the whole inteſtinal canal was moderately filled with it. The air too had penetrated into ſome parts of the cellular membrane of the ſtomach and inteſtines. The laminæ of the peritonæum, compoſing the meſentery, were ſeparated at ſome diſtance from each other by the air which occupied its cellular membrane, and the ſmall veſſels running upon the ſtomach and inteſtines were univerſally filled with it. There was about a gallon of water in the cavity of the abdomen.

Upon opening into the cavity of the cheſt, a great quantity of air was found in the cellular membrane between the pleura and the pericardium, but none could be diſcovered in the cellular membrane connecting together the air cells of the lungs. A conſiderable quantity of water was found in the cavity of the pericardium, and about two pints in the left ſide of the thorax.

What is remarkable in this caſe is, the collection of air without any external injury in the cellular membrane of any part of the body. Emphyſema generally ſpread over the body has been obſerved to take place in two ways; the one way is, by air eſcaping into the cellular membrane of the body, in conſequence of the lungs being wounded by a broken rib; the other way is, when air is generated by a putrefactive proceſs, as occaſionally happens in mortifications. Neither of theſe could have been the cauſe of emphyſema in the caſe which I have related.

1ſt. It did not ariſe from any accident, as that of a broken rib wounding the lungs. We are perfectly clear upon this point from the following circumſtances, viz. when inquiry was made, the patient could recollect no injury nor violence whatever having been lately done to her cheſt, and upon examination after death the ribs were all found to be entire.

2d. The emphyſema did not ariſe from putrefaction. There was no ſign of putridity whatever in the patient before death, and upon preſſing out the air in our examination after death from different parts, there was no fetid ſmell to be perceived, even in the ſmalleſt degree.

There are two ways in which emphyſema, not ariſing from external injury, particularly when preceded by anaſarca, may be ſuppoſed to be produced; the one is, that the watery fluids, poured into the cells of the cellular membrane, may undergo ſome chymical change, by which air is ſeparated from them; the other is, that the ſmall blood-veſſels diſtributed on the cells of the cellular membrane, may have ſome power, under particular circumſtances, of ſecreting air, which is afterwards accumulated in theſe cells. Which of the two was the cauſe in the preſent inſtance I will not poſitively determine, altho' I am inclined to think it was the latter. In caſes, however, of emphyſema, generally ariſing without external injury, I ſhould not heſitate to believe, that the moſt frequent cauſe is, the ſecretion of air by the ſmall blood-veſſels diſtributed upon the cells of the cellular membrane.

That the blood-veſſels have this power, there can be no doubt. Complete bags are found in ſome animals, containing air, being part of their natural ſtructure, as for inſtance in many fiſhes. The air could, under ſuch circumſtances, be ſeparated only by the ſmall veſſels diſtributed on the bags themſelves. Complete bags, containing air, are ſometimes found attached to the meſentery of pigs, where it could not enter *ab externo*. It is very probable alſo, that in caſes of tympanites the air is ſecreted by the ſmall veſſels opening upon the villi of the internal membrane of the inteſtines, and thrown into their cavity *.

There is no difficulty in our conceiving the poſſibility of this action of the blood-veſſels taking place; it is juſt as eaſy to conceive air ſecreted from the blood by the action of the veſſels, as the ſecretion of the bile, milk, or any other ſecreted fluid †. How far the air found in the ſmall blood-veſſels of the ſtomach and inteſtines in a dead animal is to be conſidered as a decided proof of this opinion, I will not abſolutely determine; but moſt commonly where I have found air in the inteſtinal canal, I have alſo found it in its ſmall blood-veſſels. The time taken up by this proceſs in the preſent caſe was ſhort; the emphyſema had not been obſerved by the phyſician, who had ſeen the girl two or three days before that on which I was deſired to viſit her; and Mr. Dampier, the apothecary, told me, that ſhe had ſwelled ſuddenly about a day before I ſaw her.

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* Mr. Hunter has taken notice, in his Eſſay upon Digeſtion, of a power in blood-veſſels to ſecrete air; and he has illuſtrated it by ſome of the examples here given, as well as by others.

† What is the peculiar proceſs of air being ſeparated from the blood in the blood-veſſels, I cannot pretend to ſay; but we are equally unacquainted with the peculiar proceſs which takes place in the ſeparation of any other fluid from it.

Caſes of emphyſema ariſing without external injury or putrefaction, are extremely rare. In looking over a number of books, containing accounts of caſes and diſſections, I have found only one example at all analogous to the caſe which I have related. It is deſcribed in a collection of medical caſes publiſhed by Dr. Sickel, 1744 *. In the caſe to which I allude, emphyſema is ſaid to have ſpread ſuddenly over the body of a woman, aged above forty, without any external injury whatever, and on the next day to have left her in good health.

The account which the doctor has given of the cauſe of this complaint is, that, a little before its appearance, the woman had eaten ſome muſtard-ſeed and eruca along

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[*] Vid. Sicelii Decad. quartam, p. 487.

with a ſauſage, which had expanded and ſet in motion the air contained in the blood, and other humours, in conſequence of which it was carried into the cellular membrane producing emphyſema.

I have found ſince writing this paper, that Monſ. Littre has mentioned, that where there is emphyſema from a wound in the lungs, the air taken in by reſpiration may be forced into ſmall blood-veſſels, and carried over the whole body in the general circulation, ſo as to aſſiſt in producing univerſal emphyſema. The expreſſions are as follows in the account given of his opinion:—"L'air etranger toujours pouſſé peut comme il a eſté dit, entrer dans les veines & dans les routes de la circulation, & par conſequent ſe repandre par toute l'habitude du corps *." In this opinion I believe Monſ. Littre to be miſtaken; and the air let looſe by the wound in the lungs, is ſufficient to account for the production of general emphyſema, without the aid of any other cauſe whatever. We ſuppoſe the air to be generated in the blood-veſſels themſelves, and to be thrown out in the form of a ſecreted fluid into the cellular membrane.

* Vid. Memoir. Acad. Sciences, 1713.

I have found too, that Dr. Huxham, in a letter which he has publiſhed in the Medical Obſervations, has mentioned as a conjecture the formation of elaſtic air in the blood-veſſels; but he has conſidered it as a putrefactive proceſs, and connected only with putrid diſeaſes. His words are, " and it is not improbable, that " elaſtic air may be generated even in the " arterial and venal ſyſtem, and be productive " of terrible ſymptoms, vaſt oppreſſion, an-" xiety, palpitation, intermitting pulſe, de-" liquium, &c. which are too often obſerved " towards the end of putrid fevers *."

We conſider, however, the air depoſited in the cellular membrane, in the caſe we have related, as having no connection with a putrefactive proceſs, but as being formed from the blood itſelf, by ſome peculiar arrangement of its parts, and conducted into the cells of the cellular membrane by very ſmall veſſels. The opinion, therefore, brought forward in the preſent paper, is widely different from all of theſe; we believe it to be new, and we have been chiefly led to it by what

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* See Medical Obſervations, Vol. III. p. 33. where a caſe is related by Dr. Huxham of emphyſema having ariſen during the courſe of a putrid fever and ſore throat.

Mr. Hunter has ſaid about the formation of air in his Eſſay upon Digeſtion.

Whether the air contained in the cellular membrane and the inteſtinal canal of the girl, whoſe caſe I have related, was the ſame, I cannot poſſibly determine, becauſe I did not, at the time of examining the body, think of collecting the air; it is probable, however, that the proceſs of forming air was exactly the ſame over the whole body, and therefore that the air itſelf, or the reſult of that proceſs, was the ſame likewiſe. I have twice examined the air which is accumulated in the inteſtinal canal. It was not inflammable, but, on the contrary, extinguiſhed flame; when made to paſs through lime-water, it rendered it turbid, and chalk was depoſited. The air is, therefore, either entirely of that ſort called fixed air, or at leaſt that air is in ſome very ſenſible proportion *. I am not, however, ſo much accuſtomed to chymical experiments, as to be ſatisfied with my own examination of any ſubſtance whatever in that way.

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* Dr. Pearſon has informed me, that he has examined ſome air from the inteſtinal canal very lately, and that it conſiſted of fixed and phlogiſticated air, the firſt in a ſmall, and the laſt in a much larger proportion.

XIII. *A Caſe of unuſual Formation in a Part of the Brain. By Mr.* A. Carlisle; *communicated by Dr.* Baillie. *Read Oct.* 25, 1791.

Mary Harvey, a woman apparently near twenty years of age, was admitted into the Weſtminſter Hoſpital on the 2d of July, 1791, with an ulcer affecting the tarſal bones of the foot, and her general health being much impaired, ſhe was adviſed to have the foot removed. The operation was performed by Mr. Lynn, on the 10th of July, but a conſiderable degree of fever ſupervening, together with great irritation, ſhe died about eight days afterwards. In the courſe of examining her body, which was done in preſence of Mr. Morel, one of the ſurgeons to the hoſpital, we proceeded to inquire into the ſtate of the brain; and, upon removing the dura mater from the ſuperior part of the cerebrum, a very uncommon appearance preſented itſelf. The falceform proceſs was entirely wanting; the lower edge of the triangular cavity, forming the ſuperior longitudinal ſinus, projected; but there was no portion of the dura mater whatever appended to it, and this defect prevailed equally from the criſta galli, backwards to the tentorium. A depreſſion was evident along the middle of the ſuperior part of the cerebrum, about half an inch deep, which correſponded to the lower edge of the ſuperior longitudinal ſinus; but this was ſimply a depreſſion, for there was no ſeparation of the cerebrum, as is uſual, into two hemiſpheres, it being completely formed of one ſubſtance. Although much care was taken in the diſſection, and a very ſharp knife uſed, yet not the ſmalleſt portion of pia mater could be traced in the ſituation of the natural cleft; a tranſverſe ſection was then made through the cerebrum, keeping above the level of the corpus calloſum, but ſtill no veſtiges of membrane, or of cineritious ſubſtance, or of previous inflammation or adheſion, were to be found; ſo that it ſhewed every appearance of this being the original formation. The union of what ought to have been the hemiſpheres, extended down to the corpus calloſum, which was difficult to be diſtinguiſhed from the common medullary ſubſtance of the brain. Every other part of this organ was in a perfectly natural ſtate, excepting that the opening in the tentorium, through which the cerebrum joins to the cerebellum, appeared larger than is common. The woman, previous to the operation, had her intellectual powers as perfect as people uſually have; and ſhe was not obſerved to complain of any particular affection in her head. I mention thoſe circumſtances, to ſhew how little we know of the phyſiology of the body, as connected neceſſarily with its ſtructure; for had the preſent appearance been diſcovered in diſſecting the body of a man, who had been ſubject during his life-time to any diſorder which had been ſuppoſed to depend on the brain, it is more than probable that it would have been aſſigned as the cauſe of the malady. Such caſes, however, as the above may teach us, that although the formation of an organ in the animal body ſhould be conſiderably varied from the cuſtomary plan of nature, yet the perfect and uſual function of ſuch a part may be carried on ſo well by this ſubſtituted modification, that we can hardly call it an imperfection.

XIV. *Hiſtory of a fatal Hæmorrhage from a Laceration of the Fallopian Tube, in a Caſe of an Extra-uterine Fœtus.* By John Clarke, M. D. *Phyſician to the General Lying-in Hoſpital in Store-ſtreet, and to the Aſylum for Female Orphans. Read Oct. 25, 1791.*

To Dr. BAILLIE.

Dear Sir,

I will trouble you to communicate to the Society the following very remarkable hiſtory of a caſe, which, in ſome reſpects, is different from any, which it has occurred to me to meet with in the books that I have read.

It is accompanied with a drawing of the parts: and, as a farther demonſtration of the facts, I will beg you to ſhew to the Society the preparation of the parts from which the drawing has been made, which both exhibits the ſtructure ariſing from impregnation, and alſo the lacerated ſurface, and the coagula at the extremities of the veſſels.

I am,
Dear Sir,
Yours, &c.
John Clarke.

HISTORY.

The ſubject of the following caſe was a married woman, about thirty years of age. Previouſly to the attack of the complaint which deſtroyed her, ſhe had been occaſionally ſubject to ſome bilious complaints: in all other reſpects ſhe had enjoyed a tolerably good ſtate of health, and had borne one child. About two months before ſhe died, ſhe perceived a return (as ſhe thought) of what ſhe called her bilious complaints, on account of which ſhe took, of her own accord, ſome medicines, which ſhe had been accuſtomed to employ upon ſimilar occaſions; and ſhe paid ſo little attention to an obſtruction of the menſtruous diſcharge for one period, as never to have mentioned it to her huſband, nor to her apothecary, until a few days before her death.

On the 12th of May, 1791, ſhe went into a warm bath, with a view to relieve her bilious complaints, and on Friday, the 13th of the ſame month, in the morning, without any previous exertion, ſhe was ſuddenly ſeized with a violent pain in the lower part of her belly, on account of which ſhe lay down upon a ſofa in the room, where ſhe fell into a fainting fit, from which, after a few minutes, ſhe recovered.

At this time ſhe ſent for her apothecary, a very ſenſible and intelligent man, who, as ſhe ſtill complained of great pain, took away about eight ounces of blood, ſuppoſing that there might be inflammation in ſome part of the cavity of the abdomen, and directed her to take ſome laxative medicines, in order to evacuate the inteſtines, which were attended with the deſired effect. The pain, however, ſtill remaining, he exhibited an opiate to procure relief, which it did in ſome degree, tho' not entirely.

He kept the bowels open, and palliated the pain by anodynes taken internally, thrown up the rectum by clyſter, and applied externally by fomentation, which formed the plan of treatment, and was purſued uniformly through the whole courſe of the diſeaſe.—Nevertheleſs the pain was not removed by theſe means, but returned with great violence by paroxyſms, both in the belly and near the loins, and was generally accompanied with vomiting, yawning, and fainting. The pulſe during all this time was not frequent, and the tongue was of its natural colour.

By the frequency of theſe attacks her ſtrength was very much reduced; nevertheleſs, on Monday (May 16th) ſhe thought herſelf rather better in the courſe of the day; but towards the evening her pain returned with aggravated violence, and ſhe became extremely faint and low.

At this time I was deſired to ſee her. I found her extremities in a cold ſweat:—there was no pulſe at the wriſt; ſhe was exceedingly reſtleſs, and almoſt incapable of ſpeaking (though ſhe had within a quarter of an hour been very ſenſible); ſhe became in a few minutes more and more reſtleſs, more and more faint, and expired.

After her death we were deſirous of aſcertaining by diſſection, if it were poſſible, the cauſe of this uncommon diſeaſe, which proved ſo ſuddenly fatal; and having obtained leave from her relations, we examined the body on the following day.

Upon dividing the integuments over the cavity of the abdomen, we were ſtruck with the guſhing of blood in large quantity, which, when collected, amounted to nearly a gallon. It was partly fluid, and partly coagulated. When we had cleared away the blood, the viſcera preſented themſelves not only free from any appearance of inflammation, but even more pale than they are commonly found.

We now ſought for the ſource of the hemorrhage in the ſituation of all the large veſſels near the liver, ſpleen, meſentery, &c. but in vain. Upon taking hold of the uterus, it felt more pulpy than it commonly does, and was enlarged rather beyond its natural ſize in an unimpregnated ſtate. Paſſing the hand into the cavity of the pelvis, in the poſterior chamber, to wit, between the broad ligaments and the rectum, I perceived ſtill remaining there a quantity of coagulated blood. This I removed, and, after having carefully examined all the ſurfaces, diſcovered a lacerated appearance, ſurrounded with maſſes of coagulated blood, in what at that time ſeemed to be the right ovarium. I therefore cautiouſly removed the uterus with its appendages, and reſerved them for farther and more accurate inveſtigation than could at that time be afforded.

When the blood was removed from the part (which in the confuſion occaſioned by coagula appeared to be the right ovarium) a laceration was found to be in the Fallopian tube, about an inch and an half in length, each extremity of which was about an equal diſtance from the reſpective termination of the tube in the fimbriæ and in the uterus. The diſtention of the tube at this part was nearly of the ſize of a large walnut, forming a kind of pouch. More of the coagulated blood being removed from the lacerated part, the ſhaggy veſſels of the chorion immediately appeared, interſperſed with ſmall coagula, and lying in contact with the internal ſurface of the pouch formed by the Fallopian tube: theſe being ſeparated, and the chorion divided, the amnios ſhewed itſelf, containing a fœtus perfectly formed, of about ſix or ſeven weeks growth. The Fallopian tube was pervious, both leading from the fimbriated extremity to the pouch containing the ovum, and from the pouch to the inſide of the uterus, ſo as to be capable of containing a briſtle. The diameter, or calibre of the tube between the fimbriæ and the pouch, was rather larger than it is uſually found in the impregnated ſtate. The right ovarium was the next object of examination, in which a large corpus luteum was found, occupying nearly half of the ſubſtance of the ovarium.

Laſt of all the uterus was examined. This viſcus (as I have already remarked) had a pulpy feel, and was larger than it is commonly found in an unimpregnated ſtate. Upon cutting it open, two things worthy of notice appeared. Firſt, the whole of the cervix was filled with that gelatinous matter which is not found except in the ſtate of pregnancy. Secondly, the whole of the body and fundus of the uterus was occupied by the membrana decidua, into the cavity of which the briſtles inſerted into the two Fallopian tubes penetrated.

Such were the appearances in this ſingular caſe; to which I beg the liberty of ſubjoining a few obſervations.

So many caſes of extra-uterine geſtation have been recorded by various authors, that they ceaſe to be matter of ſurpriſe. They have clearly proved the ſeat of impregnation to be in the ovarium, and have marked the progreſs or route of the ovum towards the uterus, its final deſtination. They have alſo eſtabliſhed theſe points: that the uterus has no excluſive power of forming or nouriſhing a fœtus; that, in fact, it does not poſſeſs the former property at all, and the latter only in common with other parts; but that it is only better calculated for this function by its being entirely appropriated to it, and by having an external opening for the expulſion of the fœtus.

One thing particularly deſerving of notice in the preſent caſe, is the formation of the membrana decidua in the uterus before the ovarian ovum * had reached it.

For the knowledge of this fact, which is eminently confirmed by the preſent caſe, we are indebted to the acute obſervation of Dr. William Hunter, who, in regard to the anatomical inveſtigation of the gravid uterus and its contents, ſtands high above all compariſon.

The uterus then with its appendages, the ovarium and the Fallopian tube, reſembles a piece of mechaniſm, each of whoſe parts has

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* By ovarian ovum, I mean the ovum as it comes from the ovarium, or at leaſt as it exiſts in the Fallopian tube, conſiſting of two membranes, the chorion and the amnios, which contains the liquor amnii and the embryo, but without the decidua, which membrane is formed by the uterus.

offices peculiar to itſelf, though conducing to the ſame general end.

Thus the ovarium, after impregnation, forms the fœtus, which, with its membranes, is tranſmitted through the Fallopian tube, whoſe particular function it is to catch the ovum as it comes from the ovarium, and carry it down into the cavity of the uterus. The office of the uterus is to form the decidua for the reception of the ovum, to ſecrete the mucus in the cervix, to nouriſh the fœtus during its abode there, and afterwards to expel it; ſo that each part has its own particular duty, ſeparate and diſtinct from that of all the reſt. But as each organ, tho' individually deſigned for certain purpoſes, makes a portion of the whole ſyſtem for generation, ſo it ſeems that the ſtimulus, or impreſſion for action, is communicated to the whole at once, conſequently to every part of that whole. From the application of this ſtimulus, or impreſſion for action, the uterus, as ſoon as impregnation has taken place, begins to form the decidua, in order to prepare for the reception of the fœtus. Even if the ovarian ovum ſhould not enter the uterus, the organ ſtill performs its office, having already received the ſtimulus for that purpoſe, in conſent with the whole, and independently of the fortuitous delay of the ovarian ovum, either in the ovarium itſelf, the cavity of the abdomen, or the Fallopian tube. Another circumſtance worthy of remark in this caſe is, the ſickneſs which the patient had attributed to an habitual bilious complaint. There appears to be little reaſon for doubt, that this was occaſioned by the ſtate of pregnancy; and it ſeems to prove, that the ſickneſs in pregnancy depends upon the general proceſs going on, and not on the affection of any particular part; at any rate, it has no connection with the reſidence of the fœtus in the uterus.

But the moſt remarkable thing in this caſe, is the laceration of the Fallopian tube, and the fatal hæmorrhage thereby occaſioned, eſpecially when we conſider that the patient could not have been in a pregnant ſtate more than ſeven weeks. It is difficult to account for the rupture of the tube, which ſeems to have happened ſpontaneouſly, at a time when ſhe was not only making no extraordinary exertions of body, but none at all, and one could hardly have believed, a priori, that hæmorrhage to ſuch an extent could have ariſen from the Fallopian tube, even though enlarged in its capacity, as happened in this caſe, from the growth of the ovum in it. It is plain, from a view of the pouch containing the ovarian ovum, that the veſſels are more numerous than in the natural ſtate of a Fallopian tube; yet ſtill it ſeems very ſurpriſing when we conſider that the enlarged part did not exceed the ſize of a walnut, ſo much blood could have been poured out from ſo ſmall a part.

The firſt ſenſation of pain complained of by the patient, may be probably referred to the rupture of the Fallopian tube. As both the pain and the fainting occurred by paroxyſms, it is likely that the loſs of blood was reſtrained in the intervals by the coagulation of a part of it at the extremities of the veſſels, and that when the pain was aggravated, it was owing to an increaſe of the laceration, from which more blood being poured out, the patient again fainted.

In hæmorrhages, which take place from parts expoſed to the air, perhaps the mere expoſure may have ſome effect, by ſtimulating

ing the extremities of the veſſels, to produce contractions of them, and ſo may reſtrain the loſs of blood; but in deep-ſeated parts, where the temperature is favourable to the relaxation of the veſſels, the natural means of reſtraining hæmorrhage may be exerted with more difficulty, and therefore a larger quantity of blood will be loſt than could be ſuppoſed poſſible from ſo ſmall veſſels. But this I offer entirely as a conjecture.

Upon the whole, the ſymptoms which accompanied this diſeaſe were ſuch as could not have led any one to form a conjecture reſpecting the nature of it: and even if it had been known, we could only have deplored the inſufficiency of our art to remedy a ſituation ſo uncommon and ſo fatal.

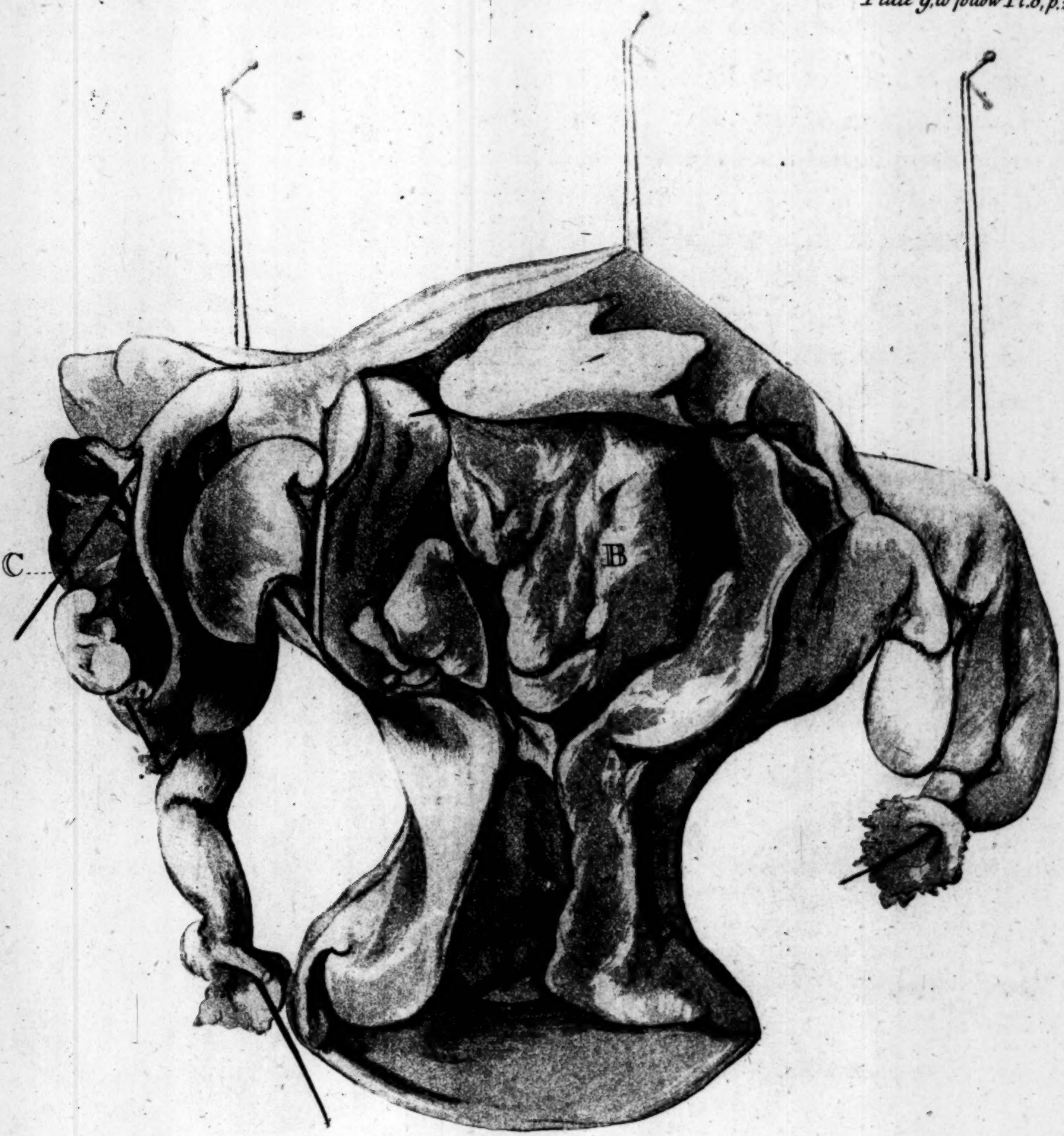
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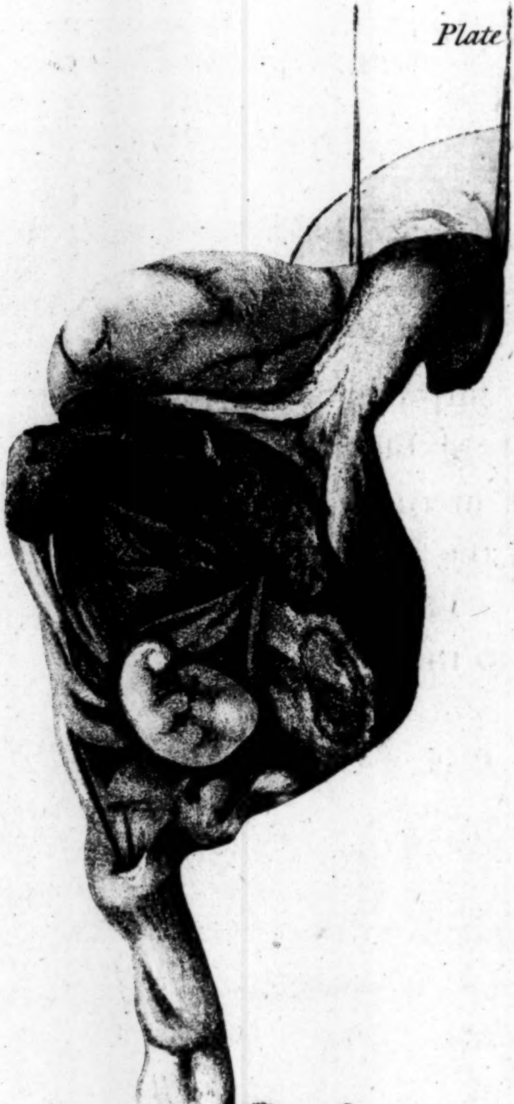
Plate 8, p. 227.



EXPLANATION *of the* PLATES.

PLATE VIII.

A view of the Fallopian tube, with the laceration in it.—The chorion and amnios are ſeen, the latter containing the fœtus.—The dark ſpots ſurrounding the chorion repreſent the coagula of blood formed at the extremities of the uterine veſſels. The briſtles are inſerted in the two extremities of the Fallopian tube. The upper leads to the uterus; the lower to the fimbriæ.

PLATE IX.

A front View of the Cavity of the Uterus.

A. The mucus in the cervix of the uterus, formed in conſequence of pregnancy, which is with difficulty expreſſed in an engraving.

B. The decidua, extending over the whole cavity of the body and fundus of the uterus.

C. A ſide view of the rupture of the Fallopian tube, and the fœtus ſeen ſuſpended in the amnios.

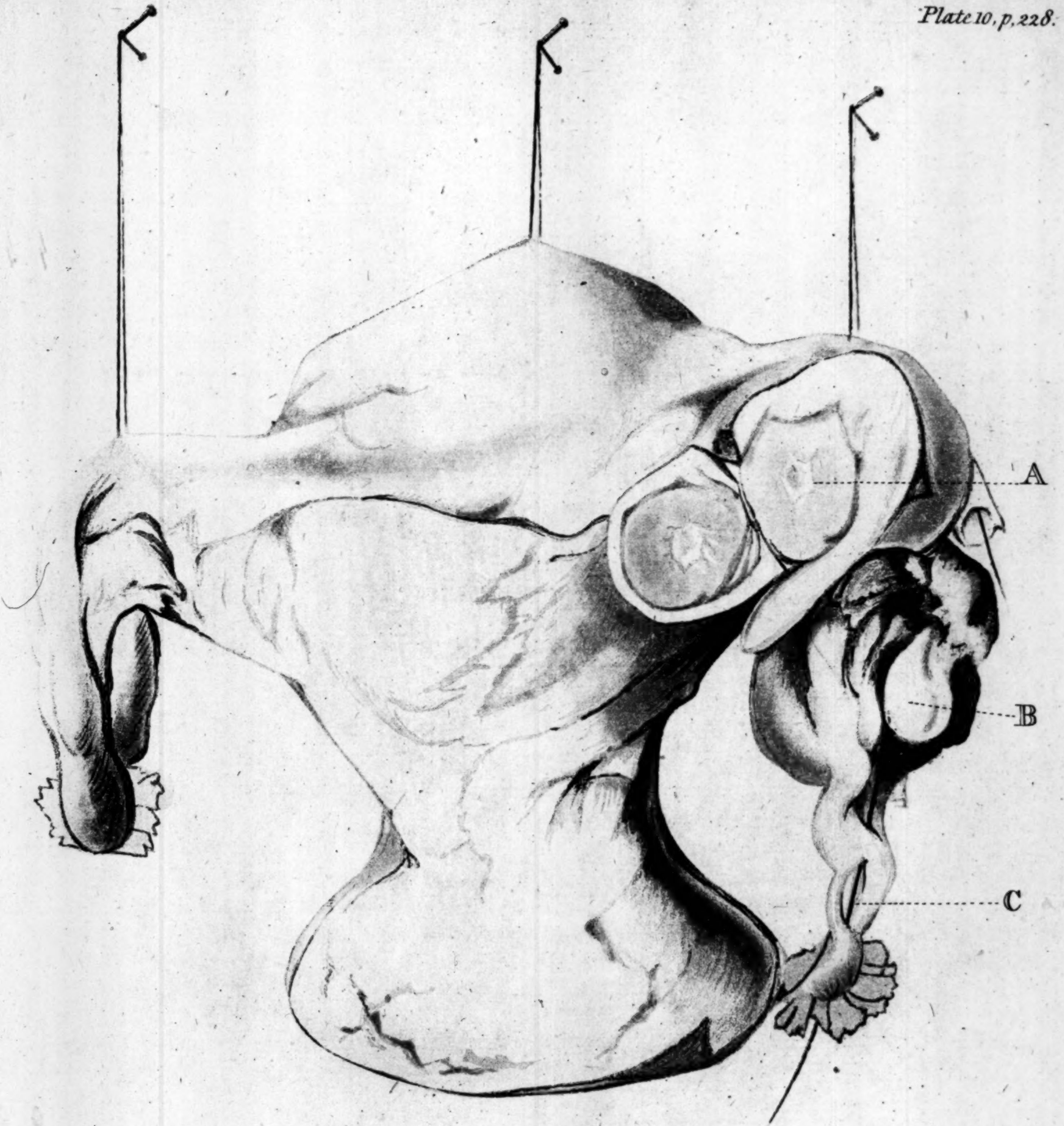
PLATE X.

A back View of the Uterus.

A. The ovarium cut open, ſhewing the corpus luteum.

B. A ſide view of the laceration in the Fallopian tube, with the fœtus obſcurely ſeen ſuſpended in the amnios.

C. A part of the cavity of the Fallopian tube ſeen through a cut made in its ſide, with a briſtle paſſing through it.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XV. *Some Obſervations on the looſe Cartilages found in Joints, and moſt commonly met with in that of the Knee. By* EVERARD HOME, *Eſq. F. R. S. and Aſſiſtant Surgeon to St. George's Hoſpital.*

SUCH detached and moveable cartilages as are the ſubject of the following obſervations, are not peculiar to the joint of the knee, they occaſionally occur in other joints of the body; but as they are moſt frequently met with in the knee, and it is in that joint they produce ſymptoms which render them the object of a chirurgical operation, I ſhall conſider them more particularly when ſituated in that cavity.

Theſe ſubſtances, in their ſtructure, are analogous to bone, but in their external appearance bear a greater reſemblance to cartilage; they are not, however, always exactly of the ſame ſtructure, being in ſome inſtances ſofter than in others. Their external ſurface is ſmooth and poliſhed, which, being lubricated by the ſynovia, allows them to be moved readily from one part of the joint to another, ſeldom remaining long at reſt, while the limb is in motion; when they happen to be in ſuch ſituations as to be preſſed upon with force by the different parts of the joint, they occaſion conſiderable pain, and materially interfere with its neceſſary motions.

The circumſtance of their being looſe, and having no remains of a viſible attachment, made it difficult to form conjectures reſpecting their formation; and I believe that no ſatisfactory account of their origin had been given, till Mr. Hunter's obſervations threw light upon the ſubject. The circumſtances which led him to the inveſtigation of this ſubject, appear at firſt ſight ſo foreign to the purpoſe, that they require ſome explanation.

In the courſe of his experiments and obſervations, inſtituted with a view to eſtabliſh a living principle in the blood, Mr. Hunter was naturally induced to attend to the phænomena which took place when that fluid was extravaſated, whether in conſequence of accidental violence, or other circumſtances. The firſt change which took place he found to be coagulation; and the coagulum thus formed, if in contact with living parts, did not produce an irritation ſimilar to extraneous matter, nor was it abſorbed and taken back into the conſtitution, but, in many inſtances, preſerved its living principle, and became vaſcular, receiving branches from the neighbouring blood-veſſels for its ſupport; it afterwards underwent changes, rendering it ſimilar to the parts to which it was attached, and which ſupplied it with nouriſhment.

In attending to caſes of this kind, he found that where a coagulum adhered to a ſurface, which varied its poſition, adapting it to the motions of ſome other part; the attachment was neceſſarily diminiſhed by the friction, rendering it in ſome inſtances pendulous, and in others breaking it off entirely. To illuſtrate this by an example, I ſhall mention an inſtance which occurred in the examination of a dead body. The cavity of the abdomen was opened, to examine the ſtate of its contents, and there appeared lying upon the peritoneum a ſmall portion of red blood, recently coagulated; this, upon examination, was found connected to the ſurface upon which it had been depoſited, by an attachment half an inch long, and this neck had been formed before the coagulum had loſt its red colour.

This ſteeped in water, ſo as to become white, appeared like a pendulous tumor.

From this caſe it became eaſy to explain the mode in which thoſe pendulous bodies are formed, that ſometimes occur attached to the inſide of circumſcribed cavities, and the principle being eſtabliſhed, it became equally eaſy for Mr. Hunter to apply it under other circumſtances, ſince it is evident from a known law in the animal œconomy, that extravaſated blood, when rendered an organized part of the body, can aſſume the nature of the parts into which it is effuſed, and conſequently the ſame coagulum which in the abdomen formed a ſoft tumor, when ſituated on a bone, or in the neighbourhood of bone, forms more commonly a hard one. The cartilages found in the knee joint, therefore, appeared to him to originate from a depoſit of coagulated blood upon the end of one of the bones, which had acquired the nature of cartilage, and had afterwards been ſeparated. This opinion was further confirmed by the examination of joints which had been violently ſtrained, or otherways injured, where the patients had died at different periods after the accident. In ſome of theſe there were ſmall projecting parts, præternaturally formed, as hard as cartilage, and ſo ſituated as to be readily knocked off by any ſudden or violent motion of the joint.

This opinion Mr. Hunter has mentioned for many years in his lectures, and his arguments in favour of it are ſo conſonant to the general laws by which the operations of an animal machine are regulated, as ſcarcely to require further evidence: but the following caſe exhibits ſo many facts in confirmation of this theory, that it appears to me to afford a full explanation of the proceſs abovementioned, and completely to eſtabliſh the opinion.

A man, ſixty-eight years of age, was brought into St. George's Hoſpital, on the 20th of March, 1791, with a ſimple fracture of the right thigh-bone. The fracture was ſituated about three inches below the great trochanter; it was treated in the uſual manner, but no bony union had taken place in the beginning of June, about eleven weeks after the accident, the portions of bone at that time being readily moved on each other. There being nothing in the man's general health to account for this backwardneſs in the parts to unite, he was deſired to explain whatever circumſtance he was acquainted with reſpecting himſelf, likely to throw any light upon it. This enquiry led the patient to mention, that his right os humeri had been broken three years and nine months before, but that the bones had continued diſunited, and admitted of motion more freely at that time, than immediately after the accident.

Reſt having proved ineffectual in producing union in the thigh, and it being evident, from the circumſtance of the arm, that there was a natural backwardneſs in the conſtitution to form bony union, he was directed to walk upon crutches, and to preſs as much upon the broken thigh as the ſtate of the parts would admit, without conſiderable pain, with a view to rouſe the parts to action, forcing them by a ſpecies of neceſſity to ſtrengthen the limb. In the courſe of a fortnight there was an evident firmneſs in the bone, and in leſs than two months the patient could walk with the aſſiſtance of a ſtick. As there was ſomething uncommon in the caſe, he was allowed to remain in the hoſpital to acquire ſtrength; in this convaleſcent ſtate he was ſeized with a complaint in his bowels, which was very violent, and carried him off.

After death the thigh-bone was found firmly repaired by bony union, but the bone of the arm, an account of which is more immediately to the preſent ſubject, admitted of motion in every direction at the fractured part.

The arm was carefully diſſected, to examine the ſtate of the fractured parts, between which there was no callus, but a large bag filled with a glary fluid, reſembling ſynovia. The internal ſurface of this bag was ſmooth, like a capſular ligament, and its attachment to the bones was of the ſame kind: it adhered firmly to the ſurrounding parts, which were thickened and conſolidated, rendering it very ſtrong. The two ends of the bone were adapted to each other, all the irregularities having been abſorbed, and their ſurfaces were of conſiderable extent, from the fracture being oblique; the upper one was ſlightly concave, or rather had two depreſſions, with a middle ridge; the lower one was ſmaller and rounded, and was adapted to both concavities, which received it in the different motions of the parts.

The ſurfaces of the bones fitted for motion were not completely covered with cartilage, but ſtudded over with it, and the bone was expoſed in the interſtices; a number of projecting parts, covered with cartilage, grew out from the ſurfaces, ſome exceedingly ſmall, others large. From the edges of the bones and the capſular ligaments, theſe excreſcences were larger, extremely irregular in their ſhape, broader in their attachments, ſofter in their texture, and ſerrated upon the external edge.

Thirty or forty ſmall ſubſtances, ſimilar to theſe above mentioned, were found looſe in the cavity, varying in ſize from that of millet-ſeed to that of a barley-corn, of a roundiſh form, and ſmooth on the ſurface; the largeſt of them were more flattened, and ſerrated. Their hardneſs varied conſiderably, ſome of them being as ſoft as cartilage, others ſo ſolid as not to be pierced by a needle. Thoſe bodies muſt have been originally attached, and broken off by the friction of the parts on one another.

The præternatural cavity which I have deſcribed, was in its nature and uſe ſimilar to the naturally formed joints of the body; theſe excreſcences and looſe bodies were its principal peculiarities, the formation of which appears to have been the reſult of the violence committed on the parts previouſly to the formation of the joint, and may be explained in the following manner.

When the bone was broken, the ruptured veſſels poured out their contents into the interſtices of the lacerated parts, for the purpoſe of uniting them again; this, however, not taking place, it was neceſſary to accommodate the parts to their diſunited ſtate, to this end the blood, which had now become uſeleſs, was in part abſorbed, and the new joint formed. The remains of the coagulated blood, which had not given the ſtimulus for its own abſorption, underwent changes in its nature, aſſimilating it as much as poſſible to that of the ſurfaces to which it was attached, in ſome parts its texture reſembling ligament, in others being more allied to cartilage, or bone.

When we compare theſe ſubſtances with the looſe cartilages found in the knee-joint, which are alſo produced in conſequence of accidental violence, and ſimilar in their appearance, we are naturally led to conclude that the latter originate from extravaſations of blood, altered in its nature by the parts in which it is depoſited, ſimilar to thoſe in the artificial joint above deſcribed. In both caſes they are evidently new formed ſubſtances, and the readieſt mode by which we can account for their production, is to refer them thus to the blood, from which fluid every part of the body was originally formed.

Theſe looſe cartilages, as they have been commonly called, although they may occur in any joint of the body, are found moſt frequently in the knee; and in this joint, from the pain and inconveniences they produce, have become the object of an operation in ſurgery.

One or more of them may be formed in the ſame joint, I have known one inſtance in which there were three; they are commonly about the ſize of a horſe-bean, often much ſmaller, and ſometimes conſiderably larger; when very large, they do not give ſo much trouble to the patient as the ſmaller kind. A ſoldier in the 56th regiment has one nearly as big as the patella, which occaſions little uneaſineſs, being too large to inſinuate itſelf into the moving parts of the joint.

In this diſeaſe, the removal of the looſe bodies is the only mode of relief; and it is fortunate for thoſe who are afflicted with it, that the knee-joint is the moſt favourable in the body for ſuch an operation; for the cavity extends a conſiderable way beyond the moving parts of the joint, and is continued into parts, which, when divided, will more readily unite than the common capſular ligaments, and be leſs liable to communicate the inflammation that comes upon the wound to the general cavity.

As theſe looſe bodies cannot always be found, no time can be fixed for the operation; but the patient, who will ſoon become familiar with his own complaint, muſt arreſt them when in a favourable ſituation, and retain them there till the ſurgeon can be ſent for.

Before the operation, the limb ſhould be extended upon a table in an horizontal poſition, and ſecured by means of aſſiſtants; the looſe cartilages are to be puſhed into the upper part of the joint above the patella, and then to one ſide, the inner ſide is to be preferred, as in that ſituation only the vaſtus internus muſcle will be divided in the operation. Should there be ſeveral of theſe bodies, they muſt be all ſecured, or the operation ſhould be poſtponed till ſome more favourable opportunity, ſince the leaving of one will ſubject the patient to the repetition of an operation, not only painful, but attended with ſome degree of danger.

The looſe bodies are to be ſecured in the ſituation abovementioned by an aſſiſtant, a taſk not eaſily performed while they are cut upon, from their being lubricated by the ſynovia; and if allowed to eſcape into the general cavity, they may not readily, if at all be brought back into the ſame ſituation.

The operation conſiſts in making an inciſion upon the looſe cartilage, which it will be beſt to do in the direction of the thigh, as the wound will more readily be healed by the firſt intention. If the ſkin is drawn to one ſide, previouſly to making the inciſion, the wound through the parts underneath will not correſpond with that made in the ſkin, which circumſtance will favour their union. The inciſion upon the cartilage muſt be made with caution, as it will with difficulty be retained in its ſituation if much force is applied. The aſſiſtant is to endeavour to puſh the looſe body through the opening, which muſt be made ſufficiently large for that purpoſe; but as this cannot always be done, the broad end of an eyed probe may be paſſed under it, ſo as to lift it out, or a ſharp-pointed inſtrument may be ſtuck into it, which will fix it to its ſituation, and bring it more within the management of the ſurgeon.

The cartilages being all extracted, the cut edges of the wound are to be brought together, and, by means of a compreſs of lint, not only preſſed cloſe to one another, but alſo to the parts underneath, in which ſituation they are to be retained by ſticking plaiſter, and the uniting bandage.

As union by the firſt intention is of the utmoſt conſequence after this operation, to prevent an inflammation upon the joint, the patient ſhould remain in bed with the leg extended, till the wound is perfectly united, or at leaſt all chance of inflammation at an end.

PLATE XI.

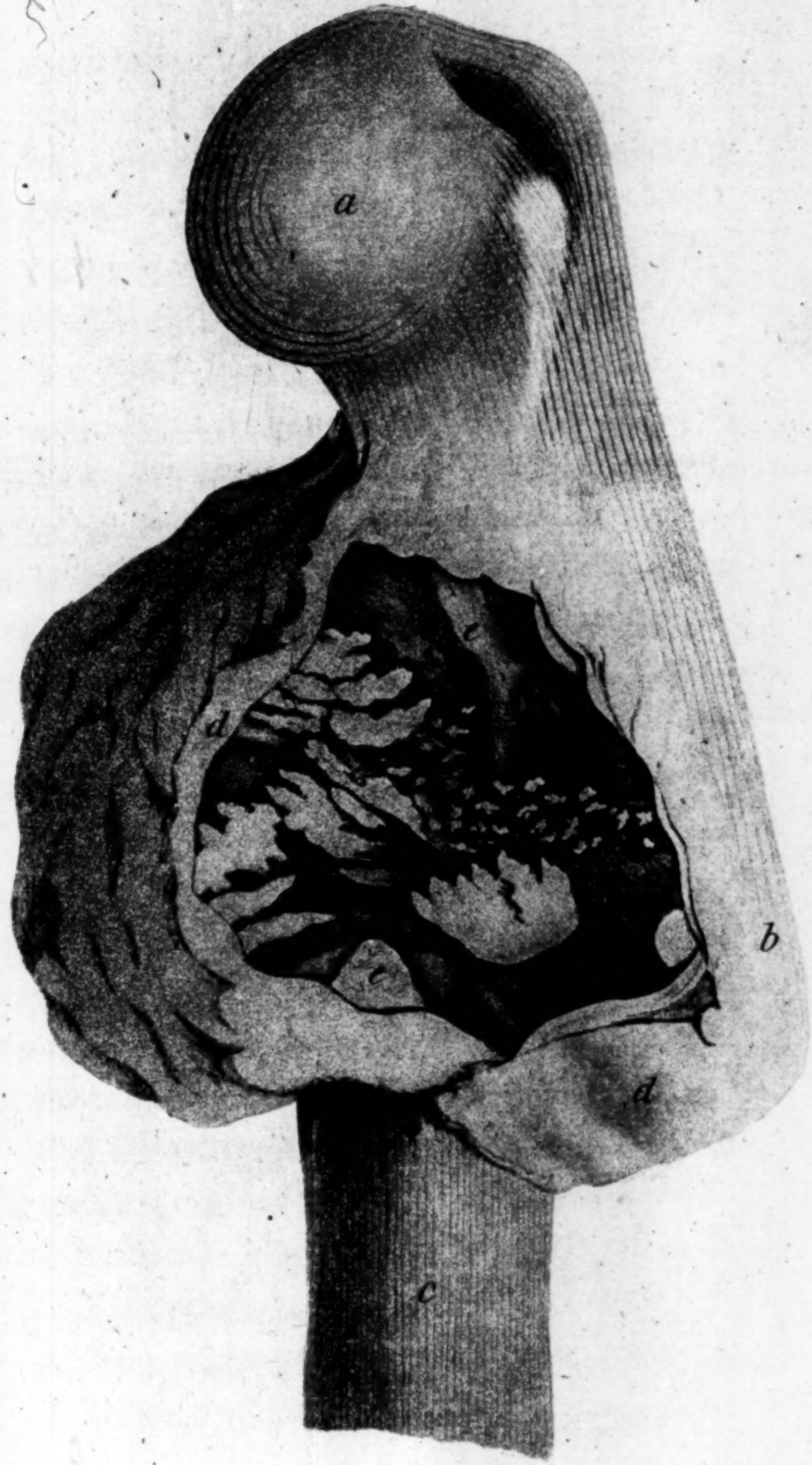
A Repreſentation of the artificial Joint.

a.	The head of the os humeri.
b.	The fractured extremity of the upper portion of the bone.
c.	The lower portion of the bone.
d. d.	The newly formed capſular ligament ſurrounding the cavity of the joint.
e. e.	The ſurfaces of the fractured ends of the bone, adapted to each other for the purpoſe of motion; the upper ſurface having two concavities, with a middle ridge; the lower one being rounded and convex, in ſome meaſure fitted to move in either of the cavities. Both theſe ſurfaces are partially covered with a ſubſtance ſimilar to cartilage, in the interſtices of which the bone is expoſed.

From the ſurface of the bones ariſe a number of ſmall hard projecting parts, very narrow at their baſe. From the inner ſurface of the capſular ligament there are excreſcences of a ſofter nature, very large, ſerrated on their external edge, and attached by narrow necks. Many of theſe ſubſtances, both of the hard and ſoft kind, were found looſe in the cavity, their attachments being broken by the motions of the joint.

XVI.

Plate 11, p. 242.



XVI. *An Attempt to improve the Evidence of Medicine.* By George Fordyce, M.D. F. R. S. *Fellow of the Royal College of Phyſicians, and Reader on the Practice of Phyſic in London.*

The evidence on which medical knowledge is founded has hitherto been principally deductions from the practice of medical practitioners, made by themſelves, and communicated to the public. The caſes themſelves from which theſe deductions were made have ſeldom been publiſhed; and when they have, they have principally been of extraordinary diſeaſes; and theſe have commonly been ſtated, with a view to ſome particular point, in ſuch a manner that they cannot be brought as evidence ſufficiently accurate to be depended upon.

The following ſcheme is intended to make the evidence in caſes more compleat, by diſſecting them, placing the progreſs of each particular ſymptom by itſelf, and ſhewing its connection with, and the relation it bears to, the other ſymptoms of the diſeaſe.

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If a perfect ſcheme of this kind was formed, and if a caſe was taken correctly according to it, it is clear that ſuch a caſe would bring into view the whole of its evidence; and that a compleat collection of ſuch caſes would form a perfect body of medicine as far as it is known. The reſult would be, that we ſhould get rid of that variety of opinion which pervades the whole of the ſcience; and our opinions would be grounded on the ſolid foundation of obſervation, and not float, as driven by the mere caprice of practitioners.

A ſecond effect a perfect ſcheme of this kind would produce, is putting the evidence which is brought in any caſe in a true point of view; and ſhowing what is ſtill wanting to make it concluſive.

In this country it is the uſage frequently to employ practitioners who are conſidered as of leſs note at the beginning of a complaint; and afterwards, if he ſhould not be thought capable of conducting, or if he ſhould not cure the diſeaſe, another is ſent for, as ſuppoſed of greater ſkill. I do not mean to argue the propriety of this uſage; but as it exiſts, it often happens that this ſecond practitioner can obtain very little authentic evidence of what went before in the diſeaſe, by which he can judge of it. It is meant as another advantage of this ſcheme to point out to theſe practitioners the things they ought to take notice of: and if they fill up the columns of this ſcheme at the time, good evidence will always be ready both to themſelves, and to any other practitioner, of what has gone before in the diſeaſe.

The things to be conſidered in a diſeaſe are divided in this ſcheme into thoſe which happen before, or at the time when, the diſeaſe takes place; and the things which happen in the progreſs of the diſeaſe.

The things which happen before and at the time of the attack are placed in horizontal columns; and under theſe the things which happen during the progreſs, in perpendicular columns.

The things which have happened before and at the time of the attack are not afterwards changed; reciting them once therefore is ſufficient.

Thoſe things which happen after the attack are conſtantly changing; all theſe are to be enumerated in the order they ariſe, and therefore muſt often be repeated. Theſe points are attained by placing them in this order, as is evident from inſpection of the ſcheme.

The firſt of the horizontal columns is the climate in which the patient lives at the time of the diſeaſe. In which we are to remark,

1ſt, The part of the globe where it lies.

The evidence of ſcience ought to be permanent; but towns and kingdoms are fluctuating, therefore the longitude of a place ſhould be taken from ſome part of the earth which is remarkable, and is likely to continue. The latitude is not liable to any error from any fluctuation that can happen for many ages.

2dly, The expoſure to the ſun.

3dly, The winds which uſually prevail; and the ſtate of the atmoſphere they produce.

4thly, The ſituation for height compared with the adjacent country, and above the level of the ſea.

5thly, The nature of the ſoil in regard to its moiſture and dryneſs; and the ſituation with regard to rivers, lakes, or marſhes.

6thly, The qualities of the waters uſed by the inhabitants, and the quantity they are ſupplied with; as that will often point out the means by which the parts of the food not uſed, as well as all other putreſcent ſubſtances, are carried off.

7thly, The temperature of the atmoſphere.

8thly, Any particular effluvia, or other matter contained in the atmoſphere in conſequence of the largeneſs of a town, mode of living of the inhabitants, mineral effluvia, &c.

9thly, The ſtructure of the houſes.

10thly, The modes of living of the inhabitants, by which is meant the uſual manners and cuſtoms of the inhabitants as to food, drinks, ſleeping, dreſs, exerciſe, and employment of time, which often gives light into the nature of diſeaſes in every part of the community.

As an example I have endeavoured to deſcribe the climate of London.

London, latitude 51° 31′ north, longitude 5′ 37″ weſt from Greenwich, 5° 16′ 23″ eaſt of the opening into the Mediterranean from the ocean. The town is large for an European town, being in a body about five miles in length, and three in breadth, beſides a number of rows of houſes lining each ſide of the roads going out from it. A mile is 1691,

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6485 lengths of a pendulum ſwinging ſeconds. The greateſt part of the town is ſituated on the north ſide of a river called the Thames. The ground on which this part ſtands is an hill, which riſes with a quick aſcent from the bank of the river, and then gradually, although unequally, to the north weſt, which is the moſt elevated part. The river on the ſouth ſide is confined by an artificial bank, the ground on that ſide being flat; but the water does not ſtagnate in the ditches, being kept in motion by the tides. On this ſide ſtands a conſiderable part of the town called Southwark, in a body near a mile both ways: above this there is at preſent only a ſmall line of the town on the ſouth ſide of the river. London reaches along the river ſide to the weſt, until the river leaves the bottom of the hill and turns to the ſouth. Above this the river is confined between artificial banks on both ſides, and is lined with houſes till it runs through two old towns, Weſtminſter and Lambeth, conſiſting moſtly of ruinous houſes, excluſive of the river, about a mile in length and breadth.

London is ſurrounded, beſides the houſes which line the roads, with many large villages; eſpecially from the weſt by the north to the ſouth-eaſt.

The river Thames runs through a valley upon a bed of gravel, and probably clay under it, for many miles above and below London. The valley is bounded on both ſides by hills not exceeding four hundred feet in height. Where the river runs in the middle of the valley, it is ſecured by artificial banks on both ſides, which have laſted longer than the memory of hiſtory, extending in all more than thirty miles. Theſe banks, when the river waſhes the bottom of the hills on either ſide, are only continued on the oppoſite ſide. When not encreaſed by either rains or the tide, the river is about a quarter of a mile broad, not more than twelve feet deep, and now and then in very dry ſeaſons it has been forded by horſes. The tide in this river riſes above fifteen miles higher than London. At London it riſes at ſpring tides from twelve to fourteen feet. The water is very pure ſome miles above the town; near the town it is mixed with mud, and contains a ſufficient quantity of mucilaginous matter to putrify. When preſerved in caſks it purifies itſelf by putrefaction, and remains afterwards more pure, but it never putrefies ſenſibly in the river, nor in the ciſterns in which it is ſometimes kept for a few days for uſe. At the lower part of the town it contains a little ſea-ſalt when the tide is at its height; but this does not reach to the middle of the town. Its ſpecific gravity is the ſame with that of diſtilled water. The inhabitants are ſupplied with this water, which is pumped up by ſeveral engines, principally worked by fire, into their houſes.

The town is alſo ſupplied with water by an aqueduct, which is brought from more than twenty miles diſtance from the north, through a canal of about ſixty miles in length. The water of this aqueduct is alſo pure, and unleſs when heavy rains bring down clay, it is bright and clear, and does not putrefy on keeping. Its ſpecific gravity is alſo the ſame with diſtilled water. This water, as well as the other, is carried in wooden pipes under the ſtreets into the houſes of the inhabitants.

There are ſprings found on digging every where in the town, which might yield large quantities of water. Theſe were formerly uſed, but are now little employed, becauſe the ſupply from the Thames and New

River is much cheaper. The waters of theſe ſprings contain a ſmall portion of ſea-ſalt, and a larger quantity of magneſia vitriolata, ſo as to be ſenſible to the taſte, and ſo as in ſome places to act as a purgative. They alſo contain gas; ſometimes in quantity ſufficient to give them briſkneſs, and render them agreeable to the taſte.

Rain water is never uſed, being always very impure from the duſt floating in the air, and lying on the tops of the houſes.

The whole ſupply of water from aqueducts and engines is 109,440 cubic feet in an hour.

The valley through which the Thames runs is gravelly, generally dry, and not marſhy till about a mile below the town, the water in the river being confined between its banks.

The hills, or riſings, on which the principal part of the town ſtands, are moſtly a mixture of clay and ſand; the ſand or gravel generally being in rather the largeſt proportion. In ſome places the ſoil is gravel.

The winds from ſouth-weſt to north-weſt, and from ſouth-eaſt to north-eaſt are the moſt prevalent. The weſterly winds blow over the great Atlantic ocean, paſſing before they reach London over part of Great-Britain for about two hundred miles, and over Ireland when they veer to the north. They are generally moiſt, although much drier than when they arrive at the weſt coaſt of the iſland. The barometer is generally low when they blow. They are commonly moſt prevalent in February, September, November and December. The eaſterly winds blow over the large continent of Europe: they are always dry. The barometer is high when they blow. They are moſt prevalent in January, March, and the beginning of April. They are cold, except ſometimes when they blow in July and Auguſt, while the weſterly winds are almoſt always warm, except in November, if they verge to the north.

The heat of the air is very variable, ſeldom remaining equal for many days; and every year differing entirely from the preceding ones, not only in heat, but alſo in moiſture, and rain. Sometimes the winter is ſeverely cold, with froſt from November till May with little interruption; ſometimes the water is not frozen for more than ten or twelve days. Moſt commonly there is a little froſt in November and December, but otherwiſe theſe months are uſually very foggy and moiſt.

The principal froſt generally is in January; February is commonly a mild, open, moiſt month. March is generally cold and dry. The ſummer months vary as much; ſometimes there are three months very warm, ſometimes not more than a week; the latter half of July is commonly the hotteſt. In Auguſt heavy rains often fall, eſpecially in the laſt half of the month. The thermometer ſometimes riſes to above 80° of Fahrenheit's ſcale, very rarely to 86°; but the moſt common ſummer heat is from 65° to 75°: it ſometimes falls in the winter to 15°; it has been known to fall below the point marked 0, but very rarely. The moſt common winter heat when it freezes is between 20° and 30°; the moſt frequent when it does not freeze between 40° and 50°.

The air when dry is always loaded with, and often obſcured by duſt, which conſiſts of aſhes and ſoot ariſing from pit-coal, the fuel which is commonly burnt; horſe dung produced, and ground to ſmall powder by the numerous carriages drawn by horſes, with which the ſtreets are always crowded; powder of granite and flints, which form the ſtreets and roads, and are ground extremely fine by the wheels of the carriages. Theſe powders, with various others, penetrate the houſes every where, and undoubtedly enter the trachea, adhere to the ſurface of the lungs, and not uncommonly produce cough, with difficulty of breathing, in people on firſt coming from the country.

The ſtreets are generally wide; few of them ſo narrow as to prevent two carriages from paſſing, and many of them wide enough to allow five or more to paſs, eſpecially in new parts of the town, which form more than half of it; they are from ſixty to twenty feet wide, thoſe of great communication ſeldom leſs than thirty, in the old part of the town; in the new part moſt of them are not leſs than forty. In ſeveral places there are ſquares of a conſiderable ſize, i. e. from about 100 to 1000 feet ſquare. The ſtreets are well paved, and clean, notwithſtanding the immenſe quantity of horſe dung conſtantly falling upon them.

The houſes, except moſt of thoſe in the oldeſt part of the town, which are not more than a fourth of the whole, have a ſtory ſunk under the level of the ſtreet. This contains the kitchen and other offices. Below the level of the bottom of this ſtory a covered canal is dug under the ſtreet, with which there is a communication from the houſes, and by which putreſcent matters ſufficiently fluid are carried off. Aſhes, bones, &c. are conveyed away in carts twice a week. The greateſt part of the houſes are of an uniform ſtructure; in each ſtory a large room in front, a ſmaller room and the ſtaircaſe occupy the back part, and there is frequently a ſmaller room added behind. There are commonly four ſtories beſides the one under the level of the ſtreet.

The town is fully inhabited; there is hardly a houſe unoccupied. The number of inhabitants is very little known: calculations of various kinds have been made by many authors; but theſe have been founded on elements entirely conjectural, and are therefore of no uſe. As far as my own opinion goes, and nothing better than opinion can be formed in this caſe, they are about a million. They conſiſt of claſſes living in very different manners.

The firſt claſs includes thoſe living on their paternal fortune, or riches ſuddenly acquired, comprehending a few merchants. The women

men of this claſs live almoſt conſtantly in their houſes, which are very cloſe, although the rooms are ſpacious, and the whole houſe perfectly clean and neat, or in carriages, with no labour and little exerciſe. This gives them a delicacy in their appearance hardly to be deſcribed. As a flower brought forward by the cheriſhing heat of a conſervatory, where it is defended from the nipping winds, exceeds any thing produced by nature alone, like it they too have a tenderneſs of conſtitution, which ſubjects them to diſeaſe from the ſlighteſt expoſure to any cauſe. Their ſituation, however, prevents them from being often expoſed to infection or ſudden cold, which are the great cauſes of violent diſeaſe in this metropolis. Their complaints therefore are generally ſlight, and very irregular; nor can they bear medicines in any way of a rough nature. Their diſorders muſt therefore be touched with the ſlighteſt hand. This has often produced an imbecility of practice, not only in London, but throughout the kingdom, which firſt infects the medical people who are immediately employed in the diſorders of this claſs. Although there may ſometimes perhaps be found one or two among theſe who are not the moſt learned or judicious practitioners, yet they are the richeſt, which contributes not a little to the ſpreading of this infection. Notwithſtanding the diſeaſes of the women of this claſs are frequent, yet they are ſeldom fatal, ſo that they often live to a great age.

The men of the firſt claſs are much in the air in the morning, and uſe exerciſe. They live in the country part of the year, when they are often occupied in hunting and ſhooting. With ſome exceptions, they are of conſtitutions ſufficiently ſtrong, are ſeldom diſeaſed; their diſeaſes are ſtrong and marked, and they bear the operation of powerful remedies.

The men who are menial ſervants of this claſs, like the domeſtic ſlaves of the ancients, are idle, lazy, uſe little exerciſe, none when they can avoid it; they are thus rendered irritable, and being often expoſed to all the inclemency of the weather, in the winter ſeaſon often till three or four o'clock in the morning, they are exceedingly ſubject to diſeaſe, particularly of the thorax; and few of them attain to any great age, except thoſe of the higher ranks.

The women ſervants reſemble in their conſtitutions their miſtreſſes.

The clergy are fewer here than in almoſt any other country in Europe. They are very apt to be affected with hypocondriacal complaints, perhaps from an idea that they do not occupy their proper rank in life. Being in general regular in their manner of living, they often attain to a great age.

The lawyers who are occupied in buſineſs, are often, from their great attention and labour of mind, weak, and diſordered in their primæ viæ. Thoſe who are not employed, may be conſidered as in the ſame ſtate with the independent gentlemen.

Phyſicians are ſo few, that it is hardly worth enumerating them. There are not much above two hundred in all, and not near half that number are employed in practice. Except when they are cut off by infectious fevers, before they are habituated to infection, although often diſeaſed, phyſicians generally attain a conſiderable age.

Attornies and apothecaries are to be conſidered in their manner of life and conſtitutions in the order of tradeſmen.

Merchants and traders of conſequence form the next claſs. The women of this claſs live a regular life, going to bed generally before midnight, and riſing about nine in the morning. Moſt families have villas near town, where the women paſs much of their time, eſpecially during the ſummer ſeaſon. They are much more in the air, and conſequently have neither the delicacy nor the irritability of the claſs we have firſt enumerated, enjoy a much better ſtate of health, their diſeaſes are more regular, and they bear the action of powerful remedies. Of the men of this claſs, ſome lead a ſedentary life, their time is much employed in writing, generally leaning on their breaſts, ſuch are ſubject to complaints in their primæ viæ; others of them uſe exercise, eſpecially on horſeback, and often ſleep in the country; all of them in point of eating are luxurious.

The leſſer tradeſmen, ſhopkeepers, and manufacturers, are alſo ſober and regular in their manner of life; but they are much confined to their houſes, eſpecially the women of this claſs, which renders them irritable and ſubject to diſeaſe, often violent and fatal. Nor is that part of the men whoſe buſineſs calls them abroad leſs ſubject to morbid affection, ſo that they rarely attain to great old age.

The laſt claſs conſiſts of the working part of the manufacturers, and labourers of all denominations; who, with ſome exceptions, are the moſt diſorderly, profligate, debauched ſet of human beings perhaps in the whole earth; working hard, and being dextrous in their occupations, and of courſe earning large ſums of money, which they ſpend in drinking, expoſing themſelves at the ſame time to the inclemency of the weather; always idle while they have any money left, ſo that their life is ſpent between labour and attention above their powers, and perfect idleneſs and debauchery. Their women, alſo paſſing from affluence to diſtreſs almoſt every week, are forced, although ſoberly inclined, to lead a very diſorderly life. Pulmonary complaints are more particularly common and fatal in this claſs, as well as all other diſeaſes.

London is fatal to infants in general. On a compariſon of charity children ſent to be nurſed in the environs, with thoſe nurſed in town, it appeared the loſs in town, being thirty-nine, was only twenty-nine in the ſame number and time in the country. But if this loſs is great altogether, it is tremendous among the loweſt claſs, the mothers being almoſt always obliged to labour for their bread, and often even robbed by their huſbands, have no time left to take the care neceſſary for the rearing of infants, ſo that they are often left to wallow in dirt, notwithſtanding the general diſpoſition to cleanlineſs in this country; and can never receive that exerciſe, or purity of the air, which is requiſite; nor can their food be at all attended to. Add to this, a pernicious practice of continuing to give them ſuck for even two or three years, by which they hope to prevent their having charge of many children. This pernicious practice goes even to ſome mothers of the ranks above this; while mothers in the higher ranks refuſe the natural ſuſtenance to their infants, leaving them often to the care of ſtrangers, regardleſs of motherly affection. From all theſe cauſes, the loſs of children in London is more than one half before they attain the fifth year of their age.

The ſecond horizontal column in the ſcheme, is marked the preceſſion of the ſeaſons, or the courſe of the preceding ſeaſons.

This column is meant principally as one ground of inveſtigating the cauſes of epidemics.

It is well known that the ſame ſpecies of diſeaſe ſometimes riſes and ſpreads through a town, a whole country, nay over a whole continent at one time; rages with violence for a certain period, and afterwards ceaſes: when this happens, we ſay the diſeaſe is epidemic. I do not mean to enter into the whole hiſtory of ſuch diſeaſes, but only to obſerve that the different ſtates of the human body ariſing from the temperature, moiſture, &c. of the atmoſphere in which a man has lived for a length of time, renders him ſubject to particular diſeaſes often more than its preſent ſtate. The inhabitants of the ſame town, nation, or even continent, ſometimes are expoſed to the ſame peculiar ſtates of the atmoſphere, and the conſtitutions of the inhabitants in general are therefore rendered equally ſuſceptible of being affected by the cauſes of the ſame diſeaſe. If ſuch cauſes, therefore, ſhould be generally applied, the diſeaſes will be univerſal, or epidemic, over the whole country. If the cauſes ſhould be partially applied, the diſeaſe will be epidemic in that town or country where they are ſo applied; and if they be applied no where, the epidemic will not ariſe, notwithſtanding the prediſpoſition.

For inſtance, dyſenteric purgings are often brought on in the autumn by the moiſture and coldneſs in that ſeaſon; but in other autumns equally cold and moiſt, dyſenteric purgings do not happen in the ſame number. In ſome they are epidemic in an high degree, in others they hardly appear. It is not, therefore, the ſtate of the atmoſphere at this ſeaſon that is to be looked on as alone producing the epidemic; for this would not have taken effect if the preceding ſummer had not given the prediſpoſition. Hot and long ſummers produce diſpoſition to dyſenteric purging. Our ſummers ſometimes begin in April, and continue till the end of Auguſt, with unequal, but ſometimes great heat through the whole; at other times they do not begin till the middle of June, and end in the beginning of Auguſt, the heat hardly at any time reaching to 70°. In the laſt caſe there are hardly any dyſenteric purgings in the autumn, but in the firſt they are very frequent and violent, eſpecially if the cold and moiſture of the autumn come on ſuddenly or by fits. But although

though the ſummer be hot and long, theſe purgings do not appear if the autumn be dry and warm.

How far we are to carry our reſearch into preceding ſeaſons is difficult to ſay; I think I have noticed effects from three years back. The years 1779, 1780, 1781, for inſtance, were years in which the ſummers were long or very hot, at the ſame time the winters were mild. It is well known that living in warm air gives prediſpoſition to intermitting and remitting fevers. At the beginning of theſe three years ſuch fevers were very rare, and hardly to be met with; they began to appear in the ſecond, and were very frequent at the end of the third, ſo that I took thirteen of them at one time into St. Thomas's Hoſpital, and theſe had ariſen in the environs of London, and we were never without many caſes of this diſeaſe *.

The courſe of former ſeaſons may be taken from journals, which are often kept with great care. Where they are kept by any public body, it is no way neceſſary for a practitioner to keep them himſelf. A clock would keep

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* Many caſes of people with intermitting fever come to the London hoſpitals from marſhy parts of the country.

them the beſt if Mr. Cummins's principle were adapted to the thermometer, quantity of rain, &c. as it is to the barometer, which might eaſily be done.

By marking the courſe of the preceding ſeaſons, it is probable that great light might be thrown on the decline of diſeaſes, as well as their ſpreading; but it muſt require a long time before much advantage will be gained.

The next column contains the preſent ſtate of the atmoſphere, which ought to be marked not only at and before the attack, but throughout the diſeaſe. This is difficult, on account of the various things to be attended to, many of which cannot be referred to public or private journals or clocks. The weight of the atmoſphere may undoubtedly; the general moiſture may alſo. The wind, which is of conſequence in this reſpect, that particular winds in particular countries have peculiar effects. The eaſt wind, for inſtance, is in this country a drying wind, and produces cold by diſſolving moiſture, ſo as to be felt in the bed-chamber of a patient in rheumatiſm, &c. as is well known. The purity of the air cannot be judged of by any thing external to the chamber in which the patient is, if he be confined to his bed or bed-chamber, with any degree of accuracy. Methods have been found lately of aſcertaining the quantity of pure air contained in the atmoſphere, but theſe methods ſhew the pure air to be in as great quantity in the atmoſphere of a ward of an hoſpital, where the infection is in the moſt violent degree, as on the banks of a ſtream murmuring over pebbles in a gravelly ſoil.

It does not ſeem to be the quantity of pure air contained in the atmoſphere that is to be ſought after, but the various ſubſtances intermixed. In the ſame manner as it is very different to the drinker of water, whether he drinks it with a duſt of plain ſand or arſenic.

The marks of purity in the atmoſphere in a room, or rather freedom from noxious vapour or powders, or ſmall drops of liquid matter, are very obſcure. A felon brought into a court of juſtice, apparently in perfect health, has, from his cloaths or perſon, ſent out ſuch a quantity of noxious matter, as to infect half the perſons preſent with violent fever. Yet the men neareſt him, nor in any other part of the court, have been ſenſible of the leaſt ſmell, taſte, feel, or any other ſenſation whatſover; ſo that unleſs it had happened

pened more than once, or even if it had not happened frequently, no man would have ſuſpected that the diſeaſe had ariſen from that cauſe. Juſt as when the venereal diſeaſe firſt broke out, no perſon ſuſpected that it aroſe from coition, but from ſome malignity of the air. The nuns therefore in Paris, in order to avoid this infection, were not cloſe ſhut up in their convents, but houſes were fitted up for them in the country, out of the infection of the air of the town.

The purity of the atmoſphere from infectious or noxious matters in the bed-chamber of a patient, muſt be taken from various circumſtances, which our preſent knowledge can give but little direction about. I have often ſeen a perſon already infected and attacked with fever, go through a fever with as mild ſymptoms in a room where there were ſeveral people ill of the ſame diſeaſe, as another who was kept in the pureſt atmoſphere that could be procured. Yet if ſeveral perſons who had entered the room had been ſeized with fever, we might determine that there was febrile infectious matter. We are, therefore, to take the purity of the atmoſphere where the patient is, more from the cloſeneſs and ſize of the room, current of air, &c. but the ſubject is not ſo far inveſtigated as to be capable of being farther gone into. The heat of the atmoſphere of the room is of material importance to be noted, both for the preſent government of the practitioner, and for the evidence of the diſeaſe. Heat in medicinal effect is not by any means to be judged of by the thermometer alone in the atmoſphere in which the patient at preſent is. There muſt be taken into conſideration the temperature he has been lately accuſtomed to. When, therefore, we ſay the air in a room is warm, we cannot fix to that idea the name of a degree in the thermometer. In winter in this country, 60° of Fahrenheit's thermometer is warm, or rather hot; in ſummer 73° is a moderate temperature. In other countries it is different. There are places in India and Africa, where 90° in certain ſeaſons of the year is cold. Having firſt aſcertained what is generally felt warm at ſeveral ſeaſons in a year, we may then refer it to the thermometer. In London in the cold ſeaſon 55° is warm, and in ſummer 73°, varying in other ſeaſons between theſe two points. It is not here proper to enter into the effect of hot and cold atmoſpheres on diſeaſes. I ſhall only mention one inſtance. In acute rheumatiſm in winter, if the heat be 60° we ſhall rarely have much metaſtaſis, while many ſhall happen if it be 45° or 50°.

Phyſicians in all ages have been of opinion, that it is of great advantage to attend to the epidemic diſeaſes which prevail, not only becauſe it is uſeful in caſes of the epidemic itſelf, but becauſe the ſame cauſes which influence the body ſo as to make it ſubject to that particular diſeaſe, render it liable to be affected in a peculiar manner when any other diſeaſe takes place. I might ground, therefore, this column upon the authority of Hippocrates, Sydenham, and various other phyſicians of great weight. But perhaps it may be uſeful to make ſome obſervations to determine what particular epidemics are more neceſſary to be noted.

Firſt then, there are certain infectious diſeaſes that become epidemical. Theſe may ariſe from an infection which produces a diſeaſe, which, when it has once happened, does not recur during the life of the perſon, ſuch as the ſmall-pox. In this caſe it may be, that a community has guarded itſelf againſt the diſeaſe ſo long, that a great number of perſons are liable to the infection. Numerous caſes of ſuch diſeaſe may ariſe on this account, if the infection ſhould be by any accident introduced into ſuch community, although there be nothing in the air or the courſe of the ſeaſons, or any other circumſtance, which renders men in general at that time more apt to receive the infection; ſuch a diſeaſe becoming epidemic in this manner is not of much uſe to be noted in this column, as it will not govern in any degree any other diſeaſe. But it frequently happens that infectious diſeaſes, whether they recur but once or oftener in the ſame perſon, ariſe and ſpread at times through a community which has not been guarded againſt them, or but negligently, as in London, where there is always ſmall-pox, and other ſuch infectious diſeaſes, which recur but once, yet ſometimes they ſpread themſelves much more univerſally than at others, and the ſame thing happens in the plague, for the more perfect hiſtory of which phyſicians are much indebted to Dr. Ruſſel, and in other diſeaſes which recur in the ſame perſon more than once. When this happens, there muſt be ſome peculiarities produced in the bodies of men in general, which renders them peculiarly liable to be infected. Theſe peculiarities may have conſiderable influence on any other diſeaſe that may ariſe, but they have not been hitherto inveſtigated with any degree of preciſion. It is, however, the obobject of this ſcheme of diſeaſe, to bring out evidence, and therefore ſuch epidemics ſhould always be noted.

Another ſet of epidemic diſeaſes, ariſing from cauſes that are very commonly applied, are not infectious, therefore the diſeaſe only ariſes more frequently in a community at a particular time, becauſe the bodies of men in general are influenced ſo as to be more eaſily affected by ſuch cauſes. Thus in the ſpring the bodies of men are rendered ſubject to phlegmonous inflammations, in the autumn to dyſenteric affections.

Or there may be diſeaſes which may ariſe either from infection or without any infection, ſuch as the eryſipelatous ſore throat, often called the putrid or ulcerated ſore throat, which is undoubtedly an infectious diſeaſe, but which likewiſe ariſes often where there can be no kind of ſuſpicion of infection. Theſe certainly become epidemic from their being peculiarities produced in the bodies of men of a community, which renders them ſubject to be affected either by infection or any other cauſe. In theſe caſes of epidemics almoſt every diſeaſe incident to the human body is more or leſs influenced by the peculiarities which occaſions the epidemic affection, and therefore ſuch require very particular notice.

The next column is meant to point out the temperament of the patient. Hippocrates conceived that there were four humours in the body; blood, bile, black bile, and phlegm: hence men, according as one or other of theſe humours abounded, were ſaid to be of a ſanguineous, bilious, melancholic, or phlegmatic temperament. This doctrine pervaded the whole of Greek medicine, and continued through the Arabian during the reign of the Abaſſides, and came, with other Greek and Arabian medicine, into Europe. The modern knowledge of the fluids of the human body, ſhews that the doctrine which treated of theſe fluids was without the ſmalleſt foundation. But it often has happened, that appearances and differences have been really marked by phyſicians both in health and diſeaſe, and have been deſcribed in words which were only applicable to the hypotheſes of the cauſes on which they were ſuppoſed to depend. Although theſe hypotheſes are not true, yet the appearances which occaſioned their formation are perfectly grounded. There does, for inſtance, exiſt in certain men, born, bred up, and living in all the circumſtances of others of the ſame community, a diſpoſition to phlegmonous and general inflammation. Thoſe who have this diſpoſition, have likewiſe their diſeaſes influenced by it, and have been ſaid to be of a ſanguineous temperament. There are others in whom perfect regularity takes place in all their diſeaſes; diſpoſition to regularity is alſo evident in their natural functions. Such have been ſaid to be of a bilious temperament. Others there are in whom there is great irregularity both in health and diſeaſe; theſe have been ſaid to be of a phlegmatic temperament. In others there is a degree of intractability in all their diſeaſes, as well as in what happens in their healthy ſtate; theſe have been ſaid to be of a melancholic temperature. It would require a much longer diſſertation than can be allowed here, to point out the various paſſages in the works of practitioners, to ſhew that this was what produced the idea of temperament. A phyſician, whoſe practice is in any degree extenſive, muſt be very inattentive not to ſee clearly theſe differences, which, however, run into one another in all kind of variety. It is much to be wiſhed, that the antient names for them were aboliſhed; in the ſpecimen, therefore, I have not uſed the term phlegmatic temperament. But whether theſe antient names may be uſed, or whether new ones are to be ſubſtituted, or general deſcription is to be given, it is a matter of great conſequence to be noted, as it often governs many circumſtances in the diſeaſe. As for example: we ſhould expect in the ſmall-pox, even before the eruption, a great degree of general inflammation during the time of the eruption, if a man had great diſpoſition to phlegmonous inflammation; but if the patient ſhould be ſubject to irregularity in natural things, we ſhould expect that the diſeaſe, when the eruption took place, would exhibit watery puſtules, ſoreneſs, great frequency of the pulſe, and the other ſymptoms of a diſeaſe which I have called, in the Elements of the Practice of Phyſick, ſymptoms of irritation.

Beſides theſe differences in the conſtitution of the body, which have been marked by phyſicians in almoſt all ages as nearly general among mankind, there are in particular habits peculiarities, which have been called idioſyncraſy, which may have influence in a great degree on the appearance of diſeaſe, and the effects of medicines. I have known, for example, many inſtances where the pulſe, inſtead of going on regularly, has been very irregular; in ſeveral caſes without any other irregularity it has intermitted, and yet all the functions of the body have been carried on perfectly. Perſons have been affected by opium, purgatives, and various other medicines, in a manner totally different from the effects which they produce in mankind generally. Such circumſtances often tend to miſlead, if they are not known; therefore, form the next horizontal column.

It is ſufficiently evident that it is neceſſary to note the uſual manner of living of a patient previous to the diſeaſe, as it has not only an influence on the diſeaſe itſelf and its appearances, but forms alſo a ground of practice. For example: if a man had been accuſtomed to drink a large quantity of wine in health, if he ſhould be ſeized with a fever, and that fever go on till the ſtrength be much exhauſted, wine might be exhibited in conſiderable quantity; while if a man who drank no vinous liquor in the ordinary mode of his living, had an equal quantity exhibited to him, he would be intoxicated. Although the faſhion of ſome practitioners at preſent is to keep patients in fevers intoxicated with wine, in as far as I have been able to judge, the practice is very detrimental. So if animal broths are to be exhibited in fevers at any time, it certainly would be more pardonable to preſcribe them to thoſe who have been much accuſtomed to live on animal food, as is the caſe of the inhabitants of this country.

In the mode of living the points to be conſidered are, the kind and quantity of food and drink which is made uſe of. The times of uſing food and drink. The defence from, or expoſure to, the weather. The quantity and times of ſleeping. The degree and times of exerciſe.

The mode of living will therefore form the next horizontal column.

The next column of thoſe containing things to be marked previous to the diſeaſe, I have called times and contingencies. Theſe are, in the firſt place, the age of the patient.

It is not of great conſequence to mark the exact age, that is, the exact number of years a man has lived; but great changes take place at different periods. The firſt period may be conſidered as happening for the firſt nine months. The ſecond from the end of that time, to the end of the fifth year. During the whole of both theſe periods, while children are ſubject to particular diſeaſes, they are remarkable for the irregularity which takes place in all their diſeaſes. The next period is from five to fourteen; the moſt healthy of the whole life-time of man, provided the ſmall-pox, and other ſuch infectious diſeaſes, which happen but once, are either gone through before, or do not occur till afterwards, and provided alſo that there is no ſcrofulous diſpoſition in the child. From fourteen to twenty-eight is the next period; during this time there is inflammatory diſpoſition in men, and to women it is a very trying one. From twenty-eight to forty-five is again an healthy period. From forty-five to ſixty is to be conſidered as the next; and after that time the ſyſtem is wearing out very faſt.

In this column the ſtate of the menſtrual flux is alſo to be noted, as well as any diſeaſe the patient has formerly been afflicted with; ſince diſeaſes have influence, in many inſtances, on thoſe which follow them, as an intermittent will occaſion a ſubſequent fever, even after years, to break down into an intermittent of the ſame type, as I have ſeen in ſeveral inſtances. Laſtly, any accidental circumſtance is to be ſet down.

The laſt thing to be noted before the taking place of the diſeaſe, is the application made to the ſyſtem which gives occaſion to the diſeaſe, and which is its occaſional cauſe. It may be an application to any part of the body, or to the mind. The knowledge of the occaſional cauſes of diſeaſes is of great conſequence for the preſervation of health. In ſo far it does not conſtitute a part of the hiſtory of a particular caſe of diſeaſe, therefore has reference to the ſcience of medicine in general. The knowledge of the occaſional cauſe is often alſo of great uſe in pointing out the treatment of diſeaſes, in as far as the maxim that the cauſe being removed the effect will ceaſe, is true.

The knowledge of the occaſional cauſe of diſeaſe is extremely difficult, and in many caſes by no means as yet aſcertained. The difficulty ariſes from various cauſes. The firſt is the uncertainty of the action of every thing upon the human body, both cauſes of diſeaſes and remedies. We have ſeen, for example, as in the caſe at the Old Bailey during Mr. Naſh's mayoralty, a felon bringing infection with him into a court of juſtice, which produced fever in a great number of thoſe aſſembled in the court, while thoſe affected ſat promiſcuouſly in every part of it with thoſe who eſcaped, and there was no human means of diſtinguiſhing any difference in the conſtitutions of thoſe infected and thoſe who eſcaped. That a man, therefore, has been expoſed to an occaſional cauſe of diſeaſe who has not been affected by it, is no proof that that particular application is not an occaſional cauſe of that diſeaſe: yet this argument has been held by many practitioners of great eminence. Sir J. Pringle, for example, affirmed that the cloaths of a man being moiſtened with water while he wore them was not a cauſe of catarrh or rheumatiſm, becauſe many men, even whole regiments of troops, have had their cloaths wet while they wore them, without either catarrh or rheumatiſm ariſing. Nevertheleſs it is certainly true, that I have ſeen more than two hundred patients at leaſt, who, when in perfect health, on having had their cloaths moiſtened while they wore them, were ſeized immediately with catarrh or rheumatiſm, when there was no other the leaſt apparent cauſe of the diſeaſe applied. On the other hand, it is equally fallacious to ſuppoſe, that any application to the body, if followed immediately by a diſeaſe, was the occaſion of it. If, for example, a man, after drinking a glaſs of water, ſhould be immediately ſeized with a fit of apoplexy, it would not follow from thence that the apoplexy was occaſioned by drinking the water. But if a diſeaſe ſhould be found to follow immediately upon any application made to the ſyſtem in a great number of caſes by one practitioner, who was the only one who had acceſs to ſee ſuch application made, there would ariſe a conſiderable degree of probability that it was the cauſe of the diſeaſe. If more practitioners than one had had occaſion to ſee ſuch application made, and found that the diſeaſe immediately followed,

lowed, the probability would increaſe. It would increaſe the more if theſe practitioners had had no communication with one another; and it would come almoſt to a certainty, if it had been obſerved by many practitioners during many ages, although the diſeaſe did not ariſe more than once in ten, twenty, or even an hundred times that the application had been made. Such, however, has the inattention to accuracy been, that many occaſional cauſes which produce diſeaſes immediately, are rather admitted from common conſent than from ſuch evidence as I have pointed out.

In the next place it happens undoubtedly, that in many caſes the occaſional cauſes of diſeaſe do not operate immediately, but the perſon appears in perfect health for ſome time before the diſeaſe comes on. As in the ſmall-pox, after catching the infection, there is undoubted proof of the patient's continuing apparently and actually in perfect health before the diſeaſe takes place. Here the difficulty increaſes in an immenſe degree. It is true, indeed, that ſuch diſeaſes as never ariſe except from one well known cauſe, ſuch as the ſmall-pox, and ſome other infectious diſeaſes, have that cauſe well aſcertained even when it is not underſtood in what manner it operated before its effects became ſenſible. But in other caſes, where a diſeaſe ariſes from many cauſes, it becomes extremely difficult to aſcertain the application which has actually produced it. This has been the origin of greater confuſion in medicine than any other circumſtance whatever: for a practitioner, in relating either a ſingle caſe, or the reſult of his experience in diſeaſe, has generally taken no manner of notice how long it was from the application of the ſuppoſed cauſe to the beginning of the diſeaſe. It is, therefore, a ſtrict rule, that in filling this horizontal column, it ſhall be ſet down exactly how long it was after the application of the ſuppoſed cauſe before any morbid appearance was perceived in the ſyſtem.

When a diſeaſe ariſes ſome time after its cauſe has been applied, it is clear that that cauſe muſt have produced ſome alteration, which became a cauſe of the diſeaſe. The conjectures with regard to this alteration, have formed the greateſt part of the fallacies, hypotheſes, and confuſion in medicine, they being almoſt entirely from conjecture, and not obſervation. It is, therefore, high time that theſe ſhould be laid aſide, and the intermediate appearances, from the application of the cauſe to the appearance of the diſeaſe, ſhould only be noted. Perhaps it may not be overloading this diſſertation, already too long, to give one inſtance. Sudden expoſure to cold certainly produces diſeaſe in many caſes. The diſeaſes which it produces have been taken for granted to ariſe ſome times at a diſtance from the time when the cold was produced, and in this caſe it has been ſuppoſed, from the matter of the inſenſible perſpiration being obſtructed, accumulated, noxious, and capable of producing diſeaſe; likewiſe that it did actually produce diſeaſes by its noxious qualities. Now of all this there is not the ſmalleſt evidence.

Theſe things are to be obſerved with regard to the evidence of the actual cauſes of diſeaſe. There is another circumſtance to be attended to; the effect which the cauſe produces, if the application of it be continued during the courſe of the diſeaſe: for ſometimes the cauſe continuing, is the occaſion of the continuance of the diſeaſe. In this caſe the maxim is true, that *cauſa dempta tolitur effectus*; as if a thorn be run into the fleſh, it produces an inflammation, and keeps it up; but if the thorn be removed before the inflammation ariſes to a certain height, the inflammation will diminiſh and ceaſe. Yet it may alſo happen, that the occaſional cauſe being removed, the effect may continue, without any application of any new cauſe produced by the firſt, and the diſeaſe may go on. It may happen in medicine as in motion, that a body being impelled by a given power, will move on, although there is no new impulſe. If the cauſe of diſeaſe continue to be applied, yet no continuance of the effect may be produced, as I have endeavoured to ſhow in a former paper in this volume on fever. Want of attention to this point has introduced an infinite number of hypotheſes in medicine, ſuch as the idea of ſome error in the fluids being the occaſion of the continuance of fever; ſuch error, or, in other words, alteration of properties can never be admitted, unleſs proved by experiments; and although I myſelf have tried various experiments to determine the properties of the fluids contained in the blood-veſſels during fever, as well as the ſecreted fluids, I never found any properties exiſting in them which do not occur in perfect health; nor has any perſon as yet found out a means of diſtinguiſhing the blood of a perſon affected by fever, from that of a man in perfect health, as far as has hitherto been made public, except that in ſome caſes of violent fever it does not coagulate ſo firmly, the red particles are broken down, and there is an evident tendency to putrefaction, even ſo as that there are inſtances upon record of its being fœtid. But this putreſcency of the blood is the effect of the depreſſion of ſtrength, for it happens only where there is previous great depreſſion of ſtrength; and when ſuch depreſſion ariſes in any other caſe, the ſame progreſs towards putrefaction is always obſerved.

There are diſeaſes, in which the diſeaſe once produced, goes on without any cauſe at all, as the motion of a maſs of matter being once produced, the maſs would continue to move on if in free ſpace.

In the next place it is to be obſerved, that this may be the caſe where the cauſes produce a diſeaſe immediately on their application, and alſo when the occaſional cauſe produces an effect, which effect is or is not a diſeaſe itſelf, but the cauſe of the diſeaſe finally produced.

I ſhall juſt add, that diſeaſes appear to go on ſimply, without any continuation of the application of any cauſe: or one part of them may be a cauſe of the other reciprocally, as ſeems to be the caſe in inflammations. But this is foreign to the preſent ſubject.

Theſe are the things to be obſerved before the diſeaſe takes place.

Thoſe things which are to be obſerved during the diſeaſe, are placed in perpendicular columns, as may be ſeen in the ſcheme, which will ſufficiently explain itſelf, as being the things laid down to be obſerved by all authors who have practiſed. There is only one column which requires explanation, the day of the diſeaſe.

A day has been variouſly conſidered, according to the purpoſes for which time has been meaſured. It is almoſt ſuperfluous to mention, that a day has been taken from the riſing of the ſun to the riſing of the ſun; from the time the ſun paſſed the meridian, till the time of its next paſſage over the meridian; from the time of a ſtar's paſſage over the meridian, until its next paſſage over the meridian. All theſe meaſurements of time, as well as others, have their particular uſes.

It is well known that almoſt all the antients, that is to ſay, the Greek phyſicians, have counted days in diſeaſe, as well as the Arabians, moſt of the moderns, the Chineſe, and indeed almoſt all nations in whom the ſtudy of medicine has made any tolerable progreſs. Yet few authors have conſidered when a medical day begins. It is true, that almoſt all have taken the period from the ſun's paſſing the meridian to the time of its next paſſing the meridian, as the length of the day, which is unequal. It ſeems however, notwithſtanding, to be received with univerſal conſent, that this inequality is not to prevent our conſidering this period as the length of a medical day. But the time of the beginning of the day is a point which, for accuracy, ſhould have been aſcertained from the firſt. Suppoſing two fevers in different perſons to begin at eleven o'clock in the forenoon on Sunday: in one of theſe the criſis takes place on the Sunday following, at eight o'clock in the morning, and in the other at two o'clock in the afternoon; if we count according to the ordinary reckoning of days, both criſes would happen on the eighth day of the diſeaſe. But if we begin the day at the time of the attack of the fever, then that criſis which took place at eight in the morning, would be on the ſeventh day; and that which took place at two, would be on the eighth. Therefore, unleſs this point be previouſly determined, every thing about critical days is thrown into confuſion. Perhaps there ſhould be two modes of counting the beginning of medical days; one from the commencement of the diſeaſe itſelf, whatever time of day it took place, which ſeems to have been adopted, and is proper, as far as I can judge, in pure phlegmonous inflammation, not attended with fever *. But in the human body a natural evening attack of fever is ſenſible in all mankind, even in the moſt perfect health, and this begins at five o'clock in the evening; therefore that hour, in many diſeaſes, muſt be accounted the beginning of a medical day.

It would lead into a diſſertation far beyond my preſent purpoſe, to lay down the obſervations from practice and contemplation of the

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* It is to be obſerved here, that I do not mean by fever, heat, and frequency of the pulſe, but what is often called ſimple fever, and by Dr. Cullen fever ſtrictly ſo called.

C A S E I. Mrs. L.

Climate	London
Preceſſion of the ſeaſons	two very long and cold winters, followed by ſummers alſo cold, but mild autumns, particularly the laſt.
Preſent temperature	
Epidemics	frequent inflammations of the ſkin, or red eruptions, ſometimes accompanied with eriſypelatous ſore throat, ſometimes without; they particularly affected that part of the town.
Temperament	rather ſmall in ſtature, and delicate. Her pulſe in health ſmall and weak, and eaſily rendered frequent.
Peculiarities	
Mode of living	as women of a middle rank; regular as to hour of ſleeping.
Times and contingencies	aged 29; menſtrua regular, not much in quantity; had been married four years without being pregnant; ſhe had been affected two years before with a nervous fever, which continued nearly four weeks.
Occaſional cauſes	none apparent.

1787 Jan.			day of diſeaſe.	Pulſe.	Skin.	Heat by the Thermometer under the tongue.	Heat feel of the patient.	Heat on touching the ſkin by the phyſician.	Muſcles.	Eyes.	Tongue.	Mouth & throat.	Reſpiration.	Stomach.	Food and drink.	Inteſtines.	Urine.	Sleep.	Miſcellaneous Obſervations.	Medicines.
6 p. m.	12	F.	1									began to feel the uvula.		appetite not loſt	refrained from animal food				excepting for the feel of the uvula no complaint	linimentum volatile gutturæ
	13	S.	1 & 2	between 80 and 90			not hot					throat ſore	natural	appetite loſt	tea and water-gruel	natural evacuation in the morning	not obſerved	diſturbed	felt generally diſordered	pulvis jacobi veſpere
	14	Sun.	2 & 3	about 100			not hot				foul	ſore, red, and beginning ſloughs	rather affected	ſick	beef-tea, barley-water, &c.	no evacuation	ditto	very little, drowſy		miſtura ſalina c. tart. emet. gr. ¼ ſextis horis
	15	M.	3 & 4	about 110			hot				fouler	ſloughs increaſed	not much affected	nauſea	the ſame	evacuation in the morning	ditto	the ſame		℞ cort. Peruv. ℈ij. tertiis hor. tinct. myrrh. ʒij. mel. roſar. ℥ij. aquæ puræ ℥viij. m. ſubinde utendum pro gargiſmate
2½ p.m.	16	T.	4	124	dry	105	hot	moderate	in no ways tenſe	dull	a thick yellowiſh brown mucus formed a triangle, the baſe to the throat	ſlough of an aſh colour covered the whole right tonſil, round which the membrane was not red	not perfectly eaſy from the affection of the throat	nauſea, which ſhe aſcribes to the medicines	ſago, panada, &c. with wine, lemonade	evacuation as uſual in the morning	ſomewhat of a whey appearance	doſing and drowſy	I ſaw her for the firſt time in this diſeaſe	℞ aq. menthi vulg. ℥jſs. cort. Per. pulv. ʒj. ſp. lavend. c. ʒj. ft. hauſt ſecunda q hora ſum. ℞ ol. olivar. ℥ij. alkal. volat. c. ℥ij. pro liniment. gutturæ applicandum ℞ tincturæ roſar. ℥vj. tinct. myrrh. ℥ij. pro garg.
noon	17	W.	5	96	moiſt and natural	100	temperate	moderate	natural	brighter	mucus cruſt leſs	membrane red about the edge of the ſlough, which begins to ſeparate	more difficult from the throat	nauſea gone, but no appetite	the ſame	the ſame	higher coloured, with a cloud	ſtill doſing and drowſy		medicines continued
																			the ſlough fell off the evening of the 18th, and ſhe recovered	

C A S E II.

Climate	London
Preceſſion of the ſeaſons	three very cold and long winters, a very warm beginning of the ſpring, but two cold months from a north-eaſt wind
Preſent temperature	cold for the ſeaſon
Epidemics	in different parts of the town different diſeaſes were more frequent. In the part where the patient lived, violent fevers and diſeaſes with ſymptoms of irritation prevailed
Temperament	bilious ſtrongly marked; complexion brown; ſtrong blackiſh hair; judgment accurate; determination great; habits fixed
Peculiarities	none
Mode of living	free but equal; dined at the ſame hour, five; eat heartily; drank a bottle of port after dinner; no ſupper; uſed much exerciſe
Times and contingencies	aged thirty-five; not married, but does not abſtain from women
Occaſional cauſes	ſat up with a friend who died of a violent fever a fortnight before the attack

1787.				Pulſe.	Skin.	Heat Therm.	Heat feel of pat.	Heat Touch.	Eyes.	Tongue.	Reſpiration.	Præcordia.	Stomach.	Food.	Drink.	Inteſtines.	Sleep.	Urine.	Occaſional Remarks.	Medicines.
June. 4 p. m.	27	W.	1ſt	130	very dry and contracted, & of a duſky colour	105	very cold	cold except in the palms and mouth	unmeaning	white mucous covers it	very difficult	tenſe	nauſea	he had lived as uſual before the attack		regular			he was ſeized at three with coldneſs, or rather a numbneſs growing into coldneſs	hora octava veſpertina ſumat ipec. gr. vj. cum tart. emet. gr. j. ℞ vin. antim. tart. ℥ij. ſumat gutt. xxx. cum hauſt. ſequent. ſextâ q. h. ℞ aq. menth. ſativ. ℥j ſs. ſp. cinnam. ʒij. ſach. pur. ʒſs. m.
1 p. m.	28	Th.	1ſt	104	contracted, not ſo dry, more of a dirty yellowiſh colour.	102	very hot	hot, and in the palms of the hands burning	a ſtupid ſtare	more ſlimy	relieved	ſtill tenſe	nauſea gone off, no nauſea	no ſolid food	only barley-water	had two evacuations in the night	a little confuſed	a white flaky ſediment	very great head-ach above the eyes, and externally at the back of his head	perſtet in uſu medicam. augendo doſin vin. antim. tart. duabus guttis ſingulis doſibus
noon	29	F.	2d	102	nearly the ſame	103	the ſame	the ſame	not altered	the ſame	the ſame	the ſame	no appetite, but medicines and drink agree		as before	no evacuation	confuſed and ſhort ſleeps	one in the morning clear. Four white flaky ſediment	head-ach continues, and he is a little delirious	repetan. hauſt & vin. ant. tart. ℞ infuſ. ſen. ℥j. ſs. tinct. ſen. ʒiij. tart. ſolub. ʒj. ft. hauſt. ſtatim ſumendus. Applicentur epiſpaſtica pone aur.
3 p. m.	30	S.	3d	106	ſame	104	complained much of the heat	not ſo burning	moiſter	browner	frequent	the ſame	no appetite without nauſea		as before	was purged twice	paſſed the night with great reſtleſſneſs, little and broken ſleep	quite tranſparent, and higher coloured	head-ach ſomewhat relieved, but confuſion continues; forty drops of the medicine made him ſick; the doſe was only thirty-eight	perſtet in uſu medicamentorum hauſtu laxante omiſſo
July. 8 p. m.	1	Sun.	5	112	dry and contracted.	105	violent	burning	more confuſed	more covered with mucus, a little browniſh	the ſame	not ſo tenſe	as before			no evacuation	had rather a better night	tranſparent thro' the whole day	head-ach but little in the morning, but is now returned	perſtet in uſu medic.
noon	2	M.	5	104	moiſt, and not much contracted	101	not very great	milder	ſomewhat more ſenſible.	ſtill covered with a dry browniſh mucus	not very uneaſy	not tenſe	the ſame	fruits, boiled barley, peaſe, potatoes	ſmall beer, barley-water acidulated	natural evacuation this morning	very confuſed the firſt part of the night, ſlept afterwards	tranſparent and high coloured, except the laſt, in which a mucous cloud	head-ach not ſo violent as in the evening; not altogether free from delirium	perſtet in uſu medic.
2 p. m.	3	Tu.	6	100	nearly the ſame	103	ſame	rather more pungent	again more confuſed	the ſame	the ſame	the ſame	the ſame	the ſame	the ſame	no evacuation	ſhort and reſtleſs throughout the night	tranſparent the whole period.	head-ach ſtill, and confuſion of ideas	perſtet in uſu med. Sumat pulv. rh. gr. xx.
1 p. m.	4	W.	7	108	dry and contracted	100	great	not very pungent	more confuſed	dry, and browner fur	more difficult	tenſe	as before	the ſame	the ſame	one evacuation this morning	none till four, ſince that time broken	quite tranſparent and high coloured	head-ach continues, and is attended with delirium, without any appearance of fulneſs of the veſſels, or fluſhing. He grew much worſe in the evening, was extremely reſtleſs, became very delirious about twelve, but grew a little more compoſed about four	nuch. applicetur epiſpaſt. hauſt veſpert. adde ſp. æther. vitriol. ʒj. veſp. extremitat. inferior. applicetur fotus ex aqua calida per horas ij.
3 p. m.	5	Th.	8	110	not quite ſo contracted	104	not ſo conſiderable	the ſame	the ſame	the ſame	not ſo difficult	tenſion leſs	as before	the ſame	the ſame	no evacuation	ſhort and broken through the night	the ſame	this night was not ſo confuſed as the preceding, but very uneaſy	perſtet in uſu medicam. injiciatur enem. è decoct. pro enem. ℥xiv. elect. len. ℥j. et ol. lini ℥j.
noon	6	F.	9	112	dry, and very much contracted	105	the patient is not ſenſible enough to judge	not ſo pungent, except in his palms	very much confuſed and wandering	the ſame	very difficult	not very tenſe, but ſome tenſion of the abdomen	cannot judge	the ſame	the ſame	two copious fœtid ſtools	hardly any	the ſame	paſſed a very reſtleſs and delirious night; the delirium is now conſiderably abated as to raving, but he is confuſed, and does not know well what is about him	perſtet in uſu medicam. omn.
2 p. m.	7	Sat.	10	108	not ſo dry	103	the ſame	the ſame	the ſame	ſtill darker brown	not ſo difficult	the ſame	no nauſea	the ſame	a little wine added	no evacuation	very much diſturbed, and not more than half an hour at a time	nearly the ſame	clyſter came away without fæculent matter	the ſame
9 p. m.	8	Sun.	12	118	very dry, and very much contracted	105	very delirious, and not able to explain	very pungent	unſteady	very dry, and ſomewhat glazed on the ſurface	frequent	great flatulency in the inteſtines, and tumor of the abdomen	does not reject nouriſhment	the ſame	milk and water for drink added to barley water	a copious and very fœtid evacuation	had a better night	in the morning a little cloud, at laſt quite tranſparent	great reſtleſſneſs and delirium, ſo that he knows nobody	℞ aq. menth. ſati. ℥j ſs. caſt. ruſſ. in p. trit. gr. x. tinct. opii gutt. viij. ſach. alb. ʒj. ſpirit. cinnam. ʒj. m. ft. hauſt ſextis horis ſumendus. Repet. enem.
1 p. m.	9	M.	12	104 hitherto has been	not very dry or	102	very delirious	not pungent	a little ſtupid	no fur, but a very	not much diſorder-	continues the ſame	the ſame	the ſame	the ſame with a lit-	the ſame	none, but great de-	a little cloudineſs		perſtet in uſu medicamen.

																	time			
9 p. m.	8	Sun.	12	118	very dry, and very much contracted	105	very delirious, and not able to explain	very pungent	unſteady	very dry, and ſomewhat glazed on the ſurface	frequent	great flatulency in the inteſtines, and tumor of the abdomen	does not reject nouriſhment	the ſame	milk and water for drink added to barley water	a copious and very fœtid evacuation	had a better night	in the morning a little cloud, at laſt quite tranſparent	great reſtleſsneſs and delirium, ſo that he knows nobody	℞ aq. menth. ſati. ℥j ſs. caſt. ruſſ. in p. trit. gr. x. tinct. opii gutt. viij. ſach. alb. ʒj. ſpirit. cinnam. ʒj. m. ft. hauſt ſextis horis ſumendus. Repet. enem.
1 p. m.	9	M.	12	104 hitherto has been much obſtructed, but now with leſs obſt. ſmall & weak	not very dry or contracted	102	very delirious	not pungent	a little ſtupid	no fur, but a very glaſſy ſurface when dry	not much diſordered	continues the ſame	the ſame	the ſame	the ſame with a little more wine, a pint in 24 hours	the ſame	none, but great delirium till five, ſlept or doſed a little afterwards	a little cloudineſs		perſtet in uſu medicamen.
2 p. m.	10	T.	13	108	moiſter	100	the ſame	the ſame	very ſtupid	the ſame	the ſame	flatulency more conſiderable	the ſame	the ſame	the ſame with beef tea	two copious fœtid evacuations	doſed the whole night, hardly ſlept	the ſame		perſtet in uſu medicamen.
1 p. m.	11	W.	14	100 and much ſofter, but weak	moiſt	98	not capable of explaining	not different from a man in health	the ſame	tongue moiſt, but rather raw	eaſy	ſtill conſiderable	the ſame	the ſame	the ſame	one, but not till the morning	ſlept two hours this morning	a conſiderable cloud, and the fluid moſt water	the firſt part of the night was paſſed with extreme difficulty, and with hardly any expectation of the patient's living till the morning, from the extreme debility and delirium	perſtet in uſu medicamen.
noon	12	Th.	15	108 but not quite regular	dry	96	the ſame	feels better	the ſame	dry and glazed	the ſame	the ſame	the ſame	the ſame	the ſame	one ſoon after the clyſter, copious, flatulent, and fœtid	ſlept three hours without interruption	a lateritious ſediment in the urine	with great difficulty the attendants can be reſtrained from giving him more wine	perſtet in uſu medicamen.

From this Time the Patient gradually recovered without any particular Criſis.

works of medical authors, to give the reaſons why I fix on five in the evening, much more ſo to enumerate the diſeaſes in which the time of the firſt attack of the diſeaſe is to be accounted the beginning of the day and the diſeaſes, in which five o'clock in the evening is to be conſidered as the beginning of the day.

There is another circumſtance with regard to the beginning of diſeaſe, which is obſerved by De Haen, to wit, that it happens in certain diſeaſes, as in fever for example, that ſome ſlight flying appearances take place before the actual attack of the diſeaſe, from whence its beginning is to be counted. It is alſo very neceſſary to pay attention to this circumſtance.

Obſervations on the Caſes inſerted in the Scheme as Examples.

It is to be obſerved, that in one caſe with which the ſcheme is partially filled up, there is a column marked the mouth and throat, which is a variable column, and is reſerved for the appearances which take place in a part of the body, if that part ſhould be the ſeat of the diſeaſe.

One of the caſes, which I have ſelected as an example, may be conſidered as not proper, on account of its not being fully deſcribed; but I choſe to take one leſs perfect, becauſe it often happens that the hiſtory of the firſt part of a diſeaſe cannot be procured.

I did not ſee the patient at the beginning of the diſeaſe, therefore could only take the firſt days from report. I alſo choſe a ſhort diſeaſe, as not having it much an object to draw off the attention to the diſeaſe itſelf from the general view of the plan. I have alſo laid before the ſociety a caſe of fever more full, and I have many others in the ſame ſtate. I have another reaſon for chuſing the firſt caſe as a caſe of eryſipelatous inflammation, in order juſt to obſerve, that although many practitioners in this town are well acquainted with the uſe of cortex cinchonæ, generally called Peruvian bark, in eryſipelatous inflammation, yet the knowledge of its virtues in ſuch inflammations is not general. It is not of much conſequence who has made any invention in the ſcience of medicine, for the ſatisfaction ariſing from being able to relieve diſtreſs by the application of a new medicine, or one already known in a new manner, or for new purpoſes, is a ſufficient recompence.

I do not mean, therefore, to diſpute the priority of the application of this medicine with any perſon who ſhall conſider himſelf as the inventor of it, I only mean to ſhow by what progreſs I have brought it in evidence that it is the moſt powerful remedy in eryſipelatous inflammation. By eryſipelatous inflammation I mean inflammation of the ſkin when it is only inflamed, or inflammation of the ſurface of an ulcer where the very ſurface only is ſore and inflamed; or inflammation of the mucous membrane, when there is little ſecretion from the mucous glands. It was in the year 1759 that I firſt applied this bark in the quantity of an ounce in twenty-four hours in a phymoſis, where there was much inflammation of the ſkin of the whole penis, having obſerved this kind of inflammation to be increaſed by evacuants. The diſeaſe gave way in leſs than forty-eight hours. Afterwards, in the year following, I juſt came to ſee my friend Dr. Balfour Ruſſel die of what is called the putrid ſore throat, or ſore throat attended with ulcers by Dr. Fothergill. Dr. Fothergill attended him from the beginning of the caſe, along with his brother. Struck with the inefficacy of the treatment by ſtimulants uſed in this caſe, and at the ſame time knowing from many caſes I had ſeen (for at that time I had applied myſelf for ten years particularly with a view to medicine) that the evacuations by bleeding, and other anti-phlogiſtic treatment, was very improper, I conceived that in this ſpecies of eryſipelatous inflammation likewiſe, Peruvian bark in large quantities might be employed with ſucceſs. I uſed it in the firſt caſe I met with of the diſeaſe, and with great advantage. As the diſeaſe was not uncommon, I had ſeveral opportunities of ſeeing its effects, which I found always ſucceſsful, and ventured accordingly to extend its uſe to other caſes of eryſipelas, ſuch as happen frequently in the face, and often in other parts of the body. I was ſo far convinced of its power, that I ventured to recommend it to my pupils, and publiſh its uſe in the eryſipelatous ſore throat in my Elements of the Practice of Phyſic, although with that diffidence a young practitioner ſhould obſerve in the introduction of any new practice. In the year 1771 I was choſen phyſician to St. Thomas's Hoſpital, where there is great opportunity of aſcertaining the efficacy of medicines in all diſeaſes. Here I have had an opportunity of verifying to myſelf as well as to my preſent colleagues, that in all caſes of eryſipelatous inflammation which are pure, that is unmixed with phlegmonous inflammation, or increaſed ſecretion from the mucous glands, even in ſome caſes of the latter intermixture, Peruvian bark is the moſt powerful remedy that can be employed, and is almoſt always ſucceſsful. It ſhould be exhibited in ſubſtance if the patient's ſtomach will bear it; and in this diſeaſe it will almoſt always bear it, and in as great quantity as the patient's ſtomach will bear, which is commonly to the quantity of a drachm every hour.

I believe there are many practitioners in this country who ſtill adhere to the treatment of eryſipelatous inflammations, and thoſe of the mucous membrane when pure, by bleeding and other evacuations, which I have always found hurtful; and I do not think the uſe of Peruvian bark is at all known abroad, excepting in gonorrhœa, from the chapter of my Text-book on the Lues Venerea being tranſlated for the uſe of the ſtudents in ſome of the medical ſchools in France *. This practice requires only to be tried to convince any one of its utility.

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* Montpelier and Grenoble.

XVII. *Obſervations, and Heads of Inquiry, on Canine Madneſs, drawn from the Caſes and Materials collected by the Society, reſpecting that Diſeaſe.* *By* JOHN HUNTER, *M.D. F.R.S. and Phyſician to the Army. At the Deſire of the Society.*

THE ſociety having collected many caſes, and other valuable information, reſpecting hydrophobia, or canine madneſs, it is judged that a ſhort abſtract of the reſult of their inquiries would not be unacceptable to the public. It will ſhew the preſent ſtate of our knowledge reſpecting that very extraordinary diſeaſe, and, it is hoped, be the means of ſuggeſting improvements both in its prevention and treatment.

I. *Of the generation of the poiſon.*

It would be of great importance, if we could aſcertain the manner in which the poiſon producing canine madneſs is generated. For ſhould it appear that it did not ariſe ſpontaneouſly among dogs, but was communicated by infection, as the ſmall-pox is in the human ſpecies, it would then be poſſible either intirely to eradicate the diſeaſe, or greatly check its progreſs, by regulations ſimilar to a quarantine. The following facts, though they will not prove that the diſeaſe never ariſes ſpontaneouſly, yet are ſufficient evidence that it is moſt commonly the reſult of infection. Dogs are more numerous in the iſland of Jamaica, than perhaps in any part of the world. It is the ambition of every negro to be maſter of a dog; yet notwithſtanding their great numbers, particularly in the towns, forty years have elapſed without a dog being known to go mad. The inſular ſituation of the country ſecures to it, in ſome degree, the advantages of a quarantine; and the rare occurrence of the diſeaſe proves that it ſeldom, if at any time, originates of itſelf. The length of the voyage from this country to Jamaica is ſuch, that it would ſcarcely be carried in the dog kind from Great-Britain, for in them the diſeaſe generally proves fatal in three weeks; but North America is much nearer, and it is from that quarter, when the diſeaſe has made its appearance, that it has been ſuſpected to come.

The moſt eminent ſportſman * in this country, to whom the preſervation of his kennel from madneſs was an object of great importance, has, for many years, preſerved his dogs from the diſeaſe, by making every new hound perform a quarantine before he was allowed to join the pack. From theſe facts we may ſafely infer, that the diſeaſe, if not always, yet certainly in the far greater proportion of caſes, even among dogs, is communicated by infection.

II. *Of the ſymptoms of the diſeaſe in the dog kind.*

From the above conſiderations we are naturally led to an inveſtigation of the diſeaſe as it ſhews itſelf in dogs, in order that it may be known early, and with certainty, and prevented from ſpreading, by the confinement or death of the animal.

Dogs ſuffer a change in their natural manners, and become dull and heavy. They grow peeviſh and ſnappiſh, and are eaſily offended. They rather take offence at ſuch things as come acroſs them, than run in ſearch of objects to attack them; yet they are more apt to quarrel with ſtrange dogs, and to run after ſuch domeſtic animals as they have been uſed to hunt, as fowls and hogs. In this ſtate the dog ſtill minds the call of his maſter, and can be reſtrained by him; but, as the diſeaſe advances, he diſregards him, appears not to know him, and will bite him if he is near him. It is probably about this period of the diſeaſe, when his recollection of perſons is nearly obliterated, that he ſtrays from home, and follows any path or road he happens to meet with, ſeemingly without purport or deſign. In this ſtate he only bites or ſnaps at ſuch animals as come in his way, giving apparently but little attention to external objects, ſo that it is eaſy to avoid him. If he be confined in this advanced ſtage of the diſeaſe, he bites and gnaws every thing near him; is furious for a moment when approached; and his chops are covered with tough frothy ſaliva. With theſe ſymptoms he ſoon expires. Before death there are ſwellings about the throat, and alſo of the tongue in ſome caſes. He can ſwallow both ſolids and liquids during the whole diſeaſe, and readily eats what is offered him. He has no fear of water, for he never avoids it. It is ſaid that dogs know, and avoid by inſtinct, one that is mad; and there appears ſome foundation for this opinion: for dogs of ſuperior courage and ſtrength have been known to run, after the firſt encounter, from thoſe that are mad, as if ſtruck with horror.

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* Mr. Meynell.

There is probably ſome variety in the diſeaſe, as it ſhews itſelf in dogs; one out of twelve or fifteen that were bit, and all died, had no marks of wildneſs or fury about him, never offered violence to any thing near him, and expreſſed pleaſure at the ſight of his maſter, by ſhaking his tail, a little before he died.

The progreſs of the diſeaſe in the dog is, in general, very uniform, yet it would appear to admit of exacerbations and remiſſions in ſome degree; for a dog, after leaving home, will return again; and, after being enraged at his maſter, will become ſubmiſſive.

There is no circumſtance that contributes more to ſpread the diſeaſe, than the diſpoſition which the dog has to leave home, and wander about; and, on this account, all ſtray dogs ſhould be approached with caution, and never with familiarity. It is a remark deſerving the moſt ſerious attention, that, in a large proportion of the caſes collected by the ſociety, the infection was communicated by taking familiar notice of ſtrange dogs.

III. *What animals communicate the diſeaſe.*

All domeſtic animals, birds, as well as beaſts, are ſuſceptible of the poiſon of the mad dog; and, indeed, our experience has not yet taught us that there is any race of animals exempted from its effects. Whether every animal labouring under the diſeaſe is capable of infecting others, or whether this power is confined to a few only, we are yet to learn.

The diſeaſe has been communicated to the human ſpecies by dogs, cats, wolves, and foxes. The dog, the wolf, and the jackal, have, by the late inquiries of naturaliſts *, been aſcertained to be of the ſame ſpecies; and therefore it is probable, from analogy, that the latter is capable of communicating the infection as well as the two former. The fox alſo has a ſtrong affinity to the dog, and is by Linnæus counted of the ſame *genus*; but the difdiſtinctions of natural hiſtory will not avail us here; for the cat, an animal of a very different *genus*, has often produced the hydrophobia in the human ſpecies. Many other animals are reported to have the power of infecting others, by biting them while labouring under the diſeaſe themſelves; but the facts hitherto collected are very vague, and lead to nothing concluſive on this head. Were we to judge from analogy, from ſeeing two animals ſo different from each other as the dog and the cat, capable of infecting others, we might be led to infer, that every animal ſuſceptible of the diſeaſe had the power of communicating it, provided their natural habits led them to bite and tear with their teeth ſuch animals as came in their way while in an enraged ſtate. But, though there are inſtances of men labouring under hydrophobia biting ſome of thoſe employed in taking care of them, no ill conſequences have been known to follow. From this, however, we can draw no poſitive inference, for it is but a ſmall proportion of ſuch perſons as are bit by dogs undoubtedly mad, who are infected with the poiſon.

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* Mr. Hunter's paper, Phil. Tranſ. vol. lxxvii. page 253.

The bite only ſerving the purpoſe of inoculation, the danger ariſing from it will be various, as it happens to be in a part more or leſs vaſcular; or as the teeth are more or leſs loaded with the poiſon. There is the greateſt danger from bites in the face, and the ſymptoms come on ſooneſt; bites in the hands alſo, which are generally bare, are full of danger. In other parts of the human body the cloathing, by wiping the teeth, greatly leſſens the danger of infection. The bite is not eſſentially neceſſary for the application of the poiſon: a dog, by licking a ſore, produced the hydrophobia; but he licked it till it bled, ſo that the poiſon came in contact with the newly divided blood-veſſels. This circumſtance, if we may judge from the analogy of other poiſons, is probably of importance in giving efficacy to the poiſon, yet it is not clear that it is eſſential; for there are two caſes of the diſeaſe mentioned in the Philoſophical Tranſactions *, which aroſe from putting the hands in the mouth of a puppy that was mad, but when there was no bite; and there is a ſimilar caſe in the † Memoirs of the Royal Academy of Sciences of Sweden. It is true, various other modes of infection are narrated by writers, but in all of them there is much appearance of fabulous credulity. There is good reaſon to think that tetanus has ſometimes been miſtaken for hydrophobia, and given riſe to the accounts of the diſeaſes proceeding from the bite of a cock, the claw of a cat, and ſimilar hiſtories.

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* Phil. Tranſ. vol. xxiii. p. 1074.

† Anno 1777.

Dogs are much more ſuſceptible of the infection than the human ſpecies. Four men and twelve dogs were bit by the ſame mad dog, and every one of the dogs died of the diſeaſe, while all the four men eſcaped, though they uſed no other means of prevention but ſuch as we ſee every day to fail. There is alſo an inſtance of twenty perſons being bit by the ſame mad dog, of whom only one had the diſeaſe.

There is a queſtion which naturally preſents itſelf here; does the diſeaſe ever ariſe ſpontaneouſly in the human ſpecies? The facts relating to this queſtion involve many doubts. Francis Stannier * died in November 1787, with the ſymptoms of hydrophobia, though it was not known that he had ever been bitten by a mad dog; and ſimilar caſes are related by writers. Yet as a large bite is no way neceſſary to communicate the infection, the patient abovementioned may have

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* London Medical Journal, vol. ix. p. 256.

have been ſlightly bit, without knowing it, as he was often drunk, and frequently in the ſtreets at night. There is good reaſon to believe that the difficulty of ſwallowing, which ſometimes occurs in tetanus, has been miſtaken for hydrophobia in ſome caſes; and there may be other ſpaſms about the throat and the œſophagus, which may ſo far reſemble hydrophobia, as to give riſe to errors on this ſubject. Something of this kind occurred once to Dr. John Hunter, in an hyſterical woman. Were we to be guided by analogy in deciding the preſent queſtion, we ſhould be led to deny the exiſtence of ſpontaneous hydrophobia; for where is there an example of any of thoſe diſeaſes which depend upon a ſpecific poiſon, as the ſmall-pox, the venereal diſeaſe, or the meaſles, ariſing ſpontaneouſly? But the full deciſion of this queſtion muſt be left to future experience and obſervation.

IV. *Effects of the poiſon on the human ſpecies.*

In obſerving the operation of the poiſon upon the human ſpecies, we find, that from the time of the bite till the diſeaſe appears, there is no derangement of health, nor any perceptible change in the conſtitution, provided the perſon bitten be not under the influence of fear. The effects of that paſſion have, in ſome caſes, been ſo great as to produce an imitation of many of the ſymptoms of the diſeaſe *; the patient actually imagining that he could not ſwallow, and ſuffering all the horrors of extreme deſpondency, till it has been diſcovered that the dog was not mad by which he had been bitten.

The wound has nothing to diſtinguiſh it from the bite of a dog which is not mad, and heals as ſoon; there are almoſt always, however, pains in and about the wound, and ſometimes ſwelling, and even a freſh diſcharge from it, ſhortly preceding the hydrophobia.

The interval between the infection and the appearance of the diſeaſe, varies from thirty-one days to ſeventeen months, in the caſes laid before the ſociety. The moſt common period appears to be about forty days. There is a caſe given in the Philoſophical Tranſactions, in which the ſymptoms appeared nineteen months after the bite †. The diſeaſe is

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* Vid. Philoſoph. Tranſ. vol. xliii. p. 257.

† Ibid. vol. xl. p. 5.

reported to occur ſometimes in a few days, as four or five, after the bite; but ſuch caſes have ſomething doubtful in them, and are probably to be referred to tetanus, or ſome other convulſive diſeaſe *.

The pains and diſagreeable feelings about the part where the wound was, are generally deſcribed as paſſing towards the head or heart, and the hydrophobia follows ſoon after. The dread of ſwallowing liquids, though the moſt ſingular ſymptom of the diſeaſe, and from which the name has been given to it, conſtitutes but a ſmall part of the malady. It is true that none, or very few, recover who have this ſymptom, yet they certainly do not die in conſequence of the difficulty of ſwallowing liquids; for the human body could eaſily exiſt double the time in which the diſeaſe uſually proves fatal, without food or drink. But the ſick can often ſwallow ſubſtances that are nouriſhing in a pulpy ſtate, without, however, having their life prolonged from it. It is not, therefore, the difficulty or impoſſibility of ſwallowing liquids, but the effects of the poiſon upon the conſtitution at large, which occaſion death.

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* Vid. Medical Tranſ. vol. ii. p. 222.

The uſual progreſs of the ſymptoms is as follows: the ſlight pains about the wound are ſoon ſucceeded by great depreſſion of ſpirits, anxiety, reſtleſſneſs, extreme ſenſibility to all impreſſions, and great difficulty, or an utter impoſſibility of ſwallowing any thing liquid. This laſt ſymptom is generally diſcovered accidentally, on attempting to drink, and, after a few trials, is accompanied with ſuch horrible ſenſations, that any thing which afterwards recalls to the mind even the idea of drinking, excites violent agitation and inexpreſſible averſion. Of the ſick who are more compoſed, and can, therefore, better deſcribe their own ſenſations, ſome ſay, that, upon attempting to ſwallow a mouthful of any liquid, they feel ſomething riſing from the ſtomach which obſtructs the paſſage; others tell you, that they have a ſenſe of choaking or ſuffocation; and it is very obſervable, that the attempt to paſs the liquid over the root of the tongue, excites convulſions in the larynx and pharynx, and even in the muſcles of the cheſt and abdomen. They complain of no pain. There are, moreover, watching, inquietude, a countenance expreſſive of extreme miſery, and fits of great irritability and almoſt fury, from trifling cauſes, as the ſick themſelves readily allow, and even wonder at when more compoſed. There is often a ſenſe of great oppreſſion at the breaſt, which the ſick generally call a difficulty of breathing, but it is properly the *anxietas præcordiorum,* which probably is an affection of the heart; for they ſigh often, and make deep irregular inſpirations, and find ſome relief from motion, as running and walking, which ſhew the lungs not to be the ſeat of the oppreſſion.

The extreme ſenſibility of the ſick to all impreſſions, appears in the diſpleaſure they expreſs at even the air blowing upon them; in their diſlike to a ſtrong light; in their averſion to new faces, or even the ſight of their friends and relations; and in the terror they expreſs at being touched, which almoſt threatens to throw them into convulſions.

As the diſeaſe advances, the mind is more filled with dreadful fears and apprehenſions; the fits of fury become both more frequent and more violent, and miſery and horror are more deeply marked upon the countenance. The ſaliva grows thick and ropy, and would appear to be ſecreted in larger quantity. The miſerable ſufferer, not daring to make the ſmalleſt attempt to ſwallow it, ſpits it out inceſſantly, which he does with much vehemence and difficulty, and with frequent retchings and vomiting. What he brings up is a little phlegm, tinged with brown or yellow bile. In this ſtate the ſlighteſt cauſe excites convulſive tremors all over the body. They ſometimes call out they are ſuffocated, turn black in the face, and expire in convulſions. It is not uncommon, however, for the ſtrength to ſink ſuddenly, the patient to become quiet and calm, as if nature gave up the ſtruggle, and to die without a groan.

The pulſe in the beginning is not quick, nor is the ſkin hot; and there is none of the muſcular debility ſo remarkable in fever; but in the progreſs of the diſeaſe there is ſome feveriſh heat, and, as death approaches, the pulſe becomes extremely quick and tremulous.

The duration of life from the appearance of the hydrophobia till death, varies from thirty-ſix hours to four or five days; the moſt common period is from two to three days.

There is conſiderable variety in the ſymptoms; they are more or leſs violent, and in proportion prove fatal in a ſhorter or a longer time. The hydrophobia, or dread of ſwallowing liquids, is in very different degrees: in ſome caſes the patient can not only make the attempt, but even force down a mouthful or two; in others, the firſt approach of any thing liquid for the purpoſe of drinking, excites the moſt violent commotion in the whole frame.

There is no part of the diſeaſe that admits of greater variation than the degree of mental derangement; which in ſome does not amount to more than extreme irritability and impatience; while in others it riſes into fits of the moſt violent rage and fury, in which they bite and tear themſelves, and every thing near them. This difference would appear to depend a great deal upon the natural temper and diſpoſition of the mind.

The diſagreeable feelings in the wound are deſcribed by the ſick ſometimes to paſs towards the head or body, and when they reach thoſe parts they produce head-ach, or pain in the breaſt. In ſome, though rarely, there is a conſiderable degree of fever. There are inſtances of the ſick having attacks of fits a good deal reſembling epilepſy. Priapiſm is a ſymptom of the diſeaſe in ſome caſes.

It ſometimes happens that all the ſymptoms admit of remiſſions and exacerbations, and that with ſome degree of regularity. Under theſe circumſtances the virulence of the poiſon would appear to be ſomewhat leſſened; and it may be a queſtion, whether the diſeaſe does not at times ſhew itſelf in a milder form, as in the caſe deſcribed by Morgagni †, and in ſome of the patients of whom the late Dr. Garden communicated an account to the ſociety.

It deſerves to be remarked as a ſingular thing in the operation of a poiſon ſo deſtructive to life, that it affects the muſcular ſtrength but little. In two caſes the panting and difficulty of breathing, as the patients called it, were remarkably relieved by running; one of them found himſelf relieved by running round Smithfield, which is nearly a quarter of a mile, a few hours before he died.

V. *Diſſections.*

Many examinations of the bodies of thoſe who have died of hydrophobia have been communicated to the ſociety. An appearance has in general been ſeen on the inner coats of the ſtomach, near the cardia, ſimilar to what is found in the bodies of perſons who have had ſlight inflammation, that is, a greater number of red veſſels with ſmall ſtreaks of red blood. In ſome inſtances there has been an increaſed vaſcularity of the *pia mater*, or ſlight watery effuſion on the ſurface of the brain. In ſome dogs that died of the diſeaſe, the appearances upon the inſide of the ſtomach were ſimilar to what have been already deſcribed; but there was no unuſual fulneſs diſcovered in the veſſels of the brain, or its membranes. Theſe appearances are no ways adequate to account for the ſymptoms of the diſeaſe.

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† De Sedibus & Cauſis Morborum, epiſt. 61. §. 14.

Morgagni *, though he has not himſelf examined the body of any that has died of this diſeaſe, yet has collected the obſervations of his friends, and compared them with ſuch as are to be found in medical writers. In no particular do they agree; and the change in the human ſtructure upon which the diſeaſe depends, would appear to be beyond the reach of anatomical inveſtigation.

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* De Sedibus & Cauſis Morb. epiſt. 8.

VI. *Of the prevention of the diſeaſe.*

We have as yet diſcovered no remedy, that has any effect in the cure of this moſt extraordinary diſeaſe; the means of prevention become, thereſore, an object of the firſt conſequence. Whatever has hitherto been done in this way is principally, if not altogether, to be referred to the local treatment of the wound, or part to which the poiſon has been applied. It is true, indeed, that numerous prophylactics have been propoſed, and many are in general uſe, which are ſuppoſed, by their effects upon the conſtitution, to counteract or expel the poiſon; yet, upon fairly weighing the proofs of their efficacy, not one appears intitled to the ſmalleſt credit. None has been held in higher eſtimation for a great length of time than ſea-bathing, though there is ſcarcely a caſe of hydrophobia publiſhed of late years in this country, that does not contain full proof of its inefficacy. The Ormſkirk medicine is in preciſely the ſame predicament. Mercury too has had a fair trial, nor has it been more efficacious than the others. It is hardly neceſſary to take notice of the inefficacy of the *pulvis antilyſſus* *, or of the Tonquin medicine †. The queſtion may here be aſked, admitting the frequent failure of theſe remedies, have they not ſometimes prevented the diſeaſe? The anſwer would certainly be in the affirmative, if every perſon bitten by a mad dog, who did not uſe ſome means of prevention, were ſeized with the diſeaſe. But this is not the caſe; for in the human ſpecies the proportion of thoſe bitten, who are ſeized with hydrophobia, is much leſs than of thoſe who eſcape, even where no means of prevention are employed: we can, therefore, infer nothing in favour of theſe prophylactics, becauſe they have been given in caſes in which no hydrophobia has ſupervened. There can be little doubt, that if in any caſe the virtues of theſe ſpecifics were ſufficient to overcome, or counteract the poiſon, they would often be ſtrong enough either to retard the appearance, or mitigate the ſymptoms of the diſeaſe, like to what we ſee from mercury in the venereal diſeaſe, or bark in intermittents; but nothing of this kind is obſerved, and the poiſon produces its peculiar effects equally ſoon and equally violent, when theſe prophylactics have been uſed, and when they have not.

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* Compoſed of the *lichen cinnerus terriſtris* & *piper nigrum.*

† Compoſed of muſk and cinnabar.

The prevention of the diſeaſe therefore depending upon the local treatment of the wound, various modes have been made trial of, either with a view to procure the diſcharge, or inſure the deſtruction, of the poiſon. Cupping-glaſſes have been applied to the wound, and it has been kept open by irritating applications for a great length of time. The part has been deſtroyed by cauſtics, and by the actual cautery; and it has been cut out. There is good reaſon to believe that all theſe have, at different times, been uſed with advantage; but the facts before the public, as well as thoſe collected by the ſociety, contain full evidence of the failure of every one of them, except the laſt mentioned, that is, cutting out the part.

In conſidering the local treatment, the following things appear to deſerve attention: 1ſt, The removal of the poiſon by waſhing the part ſimply; 2dly, The deſtroying the part by cauſtic; and, 3dly, the cutting of it out.

It has been recommended * to waſh the part with cold water repeatedly poured upon it, which is to be continued for ſome time. The practice is ſafe, and may poſſibly do good, particularly if made uſe of immediately after the bite. Cold water is better probably than warm, as warmth increaſes the ſenſibility, and quickens the action of all living parts, which might render the lacerated ſurfaces of the wound more ſuſceptible of the effects of the poiſon. Though much ſtreſs cannot be laid on this mode of prevention, yet, as caſes may occur where there may be inſurmountable objections to more effectual ſteps, it may deſerve a trial; and it would probably be more ſucceſsful, if, after waſhing copiouſly with cold water, the cauſtic alkali was to be added to the water, in ſuch proportion as the part could eaſily bear, and the waſhing to be continued with this for ſome time.

Cauſtics may be admiſſable in ſome caſes, where the knife cannot be uſed; and though they have failed in certain inſtances, yet that was probably owing to their not having been applied to all the infected ſurfaces. Late ex-

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* London Medical Journal, vol. x. p. 295.

periments * have indeed taught us, that all cauſtics do not counteract the effects of poiſons applied to the human body. It would appear, however, that the beſt cauſtic has not been uſed, which is probably the cauſtic vegetable alkali in a ſolid form, the *kali purum* of the London Pharmacopœia; both as it acts more ſpeedily, and alſo more completely deſtroys and diſſolves all animal ſubſtances. By applying it to the various ſurfaces of the wound, and immediately removing with a ſpatula the parts upon which it has acted and diſſolved, it may again be applied, and the part deſtroyed to what depth may be deemed neceſſary: the ſurgeon ſeeing all the time the extent to which the cauſtic goes.

What has been ſaid under the two preceding heads on waſhing off the poiſon or deſtroying it by cauſtic, muſt be underſtood to apply only to thoſe caſes where there are inſurmountable objections, either from the place where the bite is, or other cauſes, to cutting out the part. Whenever the part can be cut out, it ought always to be done, as the only certain means hitherto employed of preventing the diſeaſe. In performing this operation care muſt be taken to cut out every part with which the dog's teeth have come in contact, for the ſmalleſt portion left might produce the diſeaſe. It becomes, therefore, neceſſary for the operator to examine accurately the part, and to aſcertain to what depth, and in what direction, the teeth have penetrated, keeping in his mind the ſituation of the parts in the act of biting, in which the ſkin and the fleſh are pinched up, and therefore put out of their natural poſition before the teeth penetrate them *.

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* Vid. Fontana, vol. ii. p. 5. and Appendix.

The ſooner the operation is performed it muſt be ſo much the ſafer; but there ariſes a queſtion of conſiderable importance, that is, how long after the bite can the part be cut out, ſo as to prevent the diſeaſe? Nothing could determine this fully but experiments made upon the poiſon, an arduous undertak-

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* Mr. Hunter in one caſe removed the parts, as he thought, freely, and there was nothing on the under ſurface of the piece cut out, that led him to ſuppoſe he had not cut beyond the bite. But on examining the ſurface of the wound, he found a part in the middle which was hollow underneath, which ſhewed he had not gone deep enough, but had left a ridge, as it were, over part of the paſſage made by the dog's teeth, and which could only have been diſcovered by examination after the operation. This is mentioned, to ſhew the care that is requiſite in cutting out the part, and alſo the neceſſity of going to a proper depth.

ing, and not without much danger; yet not to be deſpaired of, if we advert to the experimental inquiries concerning poiſons, which a thirſt after knowledge has produced of late years. Till ſomething of that kind is done, the queſtion can only be anſwered imperfectly from a few ſcattered facts, and conjectures. From the facts before the ſociety it appears, that the part has been cut out * thirty hours after the bite, and in one caſe as late as the fifth day.

In one of the patients, an account of whom has been communicated to the ſociety, the bite immediately inflamed and feſtered, and on the fifth day an abſorption took place, as appeared by the ſwelling of a gland in the arm-pit, which was followed by a conſiderable degree of fever, that laſted ſome days; but none of the ſymptoms of hydrophobia appeared till the uſual period, and after ſigns of a ſecond abſorption, when the diſeaſe proved fatal. From this and ſimilar facts in the animal œconomy, it is probable that the poiſon lies long in the part, as in ſome other caſes of inoculation, and would ap-

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* Dr. John Hunter directed the part to be cut out, and it was done in his preſence by Mr. Foote, who has publiſhed the caſe, but appears to have forgot the operation, for he ſtates that the tooth perforated the part of the ſkin cut out, which if it had, the operation would have been of no uſe.

pear to be incapable of affecting the conſtitution, till it had produced a previous change in the part. If a conjecture might be allowed on this point, it would ſeem that the pains about the wound riſing towards the head or body, indicated the abſorption of the poiſon; and that the poiſon had been confined to the wound till a day or two before the ſymptoms of the diſeaſe appeared. On this ſuppoſition the part might be removed, even weeks after the bite: but we muſt wait for further information and experience before we can come to a poſitive concluſion on this head.

VII. *Of the treatment of the diſeaſe.*

After the ſymptoms of hydrophobia have appeared, no medicine or remedy that has hitherto been uſed has relieved, much leſs cured the diſeaſe. On this conſideration it may ſeem unneceſſary to enumerate the various things that have been tried; but, without a knowledge of what has been attempted, much time muſt be loſt in uſeleſs trials and fruitleſs repetitions; and in fact, the want of preconcerted order and method in inveſtigating the cure of this diſeaſe, is very apparent in all the writers upon the ſubject, and is probably a principal cauſe that no progreſs has been made in that important enquiry. In ſtating ſhortly the various remedies that have been tried, opium deſervedly claims the firſt place. Reaſoning from analogy, there is no article in the materia medica that would appear ſo peculiarly adapted to the relief or cure of all the ſymptoms of hydrophobia, as opium. It has therefore, in various ſhapes and forms, had the moſt full and ample trial, and has been found to do no good; and it can only be imputed to the want of method and order abovementioned, that this medicine ſtill continues to be given in almoſt every caſe of this diſeaſe. It is evident that, however much might have been expected originally from opium, there are at preſent few articles in the catalogue of medicines, that have not a better chance of curing hydrophobia.

Mercury has alſo had a fair trial, both with a view to prevent the diſeaſe, and likewiſe to remove it after it had actually taken place. But in neither caſe has it done any good.

Bleeding, the warm bath, alſo bliſters and cantharides have been uſed, but with no better effect.

In order, therefore, to avoid a repetition of medicines, which can be productive of no good, we ought to have preconcerted various new methods of treating the diſeaſe, which may be tried as circumſtances may indicate. In moſt diſeaſes the doctrine of the *juvantia* and *lædentia*, as it has been called, affords ſome direction reſpecting the treatment; in hydrophobia there is ſcarcely any thing obſerved to afford even a temporary relief. Among the caſes collected by the ſociety there are two, in which the relief obtained by running was very remarkable; in one the amendment was ſo conſiderable, that the patient did not look like the ſame perſon after running about a quarter of a mile. This ſuggeſts an experiment, which might be eaſily tried. It would probably be made to beſt advantage in the open air, in a quiet, retired, and ſhaded place. It would ſoon appear, whether it were better to run briſkly for a ſhort time, with frequent ſtops between; or to take a ſlower pace, that could be continued for a greater length of time. If the patient found relief, there would be no difficulty in getting him to perſevere.

All the caſes of the diſeaſe laid before the public contain proofs of the ſufferings of the patients in attempting to ſwallow liquids; it is, therefore, an experiment that ſhould not be wantonly, and out of mere curioſity, made upon them. At the ſame time it is an object of much conſequence to ſupport by nouriſhment and cordials a body labouring under the influence of ſo deſtructive a poiſon. Food, therefore, in a pulpy ſtate, ſhould be got down as much as poſſible; and if mixed with wine it would probably be the better. Bread or biſcuit ſoaked in wine would anſwer this deſcription. But if nouriſhment could not be got down, broths, or warm wine and water, might be adminiſtered by clyſter. Some plan of this kind would be abſolutely neceſſary, if motion was found to give conſiderable relief.

It has been ſtated, that the means of prevention conſiſted chiefly in the local treatment of the wound; and it may be a queſtion how far ſomething of the ſame kind might not prevent an aggravation of the ſymptoms, after the diſeaſe has come on. There is reaſon to ſuſpect that the poiſon is confined to the part bitten till within a very ſhort time of the appearance of the diſeaſe; and probably the moment the poiſon is conveyed into the general circulation by the abſorbents, the ſymptoms begin. Judging from the analogy of ſome other poiſons, the greater quantity that is conveyed into the blood the more violent will the effects be; good, therefore, might ariſe from leſſening this quantity. With this view numerous and deep ſcarifications might be made where the wound was, and cupping-glaſſes applied repeatedly. Ligatures may be applied. Might not ice, or a freezing mixture be applied ſo as to benumb the part, and arreſt for a time all motion, and of courſe that of the lymphatics?

There is ſometimes an appearance of periodical remiſſions and exacerbations in the ſymptoms; and there is ſome reaſon to ſuſpect that ſuch caſes are of a milder nature, and that ſome of them recover. Upon this ground the Peruvian bark might be tried. It might be ſwallowed in an electuary, and alſo thrown up by clyſter.

In the Eaſt-Indies, in the Carnatic, there is a pill in uſe, ſaid to be a ſpecific againſt the poiſon of ſerpents, and alſo the bite of a mad dog. The principal ingredient has been aſcertained to be arſenic. It may ſeem ſtrange to propoſe as a remedy in any caſe, a ſubſtance that is a moſt deadly poiſon; but we know that arſenic can produce not only very great, but alſo very ſalutary effects upon the conſtitution. The moſt obſtinate intermittents have been cured by a few drops of a ſolution of it. It might be tried in the ſame form, and in a doſe ſomewhat increaſed, in hydrophobia, as it is in agues. One tenth of a grain diſſolved in water is given in agues three times a day; this doſe might be doubled.

It would not be difficult to enumerate various other active ſubſtances in the materia medica, which might be tried againſt the hydrophobia; but every one may readily chuſe for himſelf; for our grounds of trying one in preference to another are merely hypothetical.

Nothing would probably contribute ſo much towards aſcertaining a proper treatment of the diſeaſe, as experiments made upon the poiſon. It is true the undertaking would be both difficult and hazardous; yet the danger is probably greater in apprehenſion than it would be in reality. Dogs known to be mad are often confined till they die; and in this ſtate there would be no great difficulty in collecting a portion of the ſaliva, or poiſon. The point of a lancet would probably introduce the infection as certainly as the tooth of the dog. By experiments upon dogs, the ſeveral ſtages of the diſeaſe might be known; the lateſt period at which the contaminated part could be cut out with ſucceſs, might be aſcertained; and various *counter poiſons* might be applied to the wound, or adminiſtered internally. It might be determined, whether the ſaliva of other animals, beſides thoſe formerly mentioned, had the power of communicating the diſeaſe, while labouring under it. An experiment with the ſaliva of an hydrophobic patient might eaſily be made upon a dog. It might be aſcertained whether any of the humours of the body of an infected animal poſſeſſed the poiſonous quality, except the ſaliva. The liver of a dog that has died mad is reputed to be a ſpecific againſt the poiſon, and as ſuch is eaten in ſome parts of the world; but we cannot abſolutely infer from this that it is exempt from the poiſon, for the digeſtion of the ſtomach deſtroys ſome poiſons.

VIII. *Of the antiquity of the diſeaſe.*

In the days of Plutarch * it was a queſtion, whether canine madneſs were not a new diſeaſe. The ſame point is diſcuſſed by Cœlius Aurelianus †; but their mode of treating the queſtion affords little ſatisfaction to a modern enquirer; their arguments turn upon ſpeculative and metaphyſical points, and they neglect to examine into the matter of fact, the only uſeful information. But though we are much farther removed from the origin of the diſeaſe, ſuppoſing it to be a new one, we are probably in a better condition to determine the queſtion, from the ample experience that later times have furniſhed of new diſeaſes. In the courſe of the laſt eight or nine centuries there have appeared many new diſeaſes; and it is remarkable, that they are not only all infectious diſeaſes, but are capable of being produced by infection alone, at leaſt as far as our experience goes. Of the infectious nature of canine madneſs there can be no doubt, and there is reaſon to ſuſpect, as has been ſtated above, that it is only communicated by infection; in both particulars, therefore, it agrees with what may be called the new diſeaſes of modern times. The queſtion ſimply of the novelty of a diſeaſe muſt be a thing of little conſequence, but when that is connected with the manner in which it is propagated, it becomes a conſideration of importance; and if it ſhould appear that there is reaſon for believing the hydrophobia to be a new diſeaſe, it may be an additional proof of the opinion already advanced, that it is only propagated by infection.

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* Sympoſiacon, lib. viii. probl. 9.

† Acut. Morb. lib. iii. cap. 15.

It is ſaid in Plutarch, that the hydrophobia was firſt ſeen at Rome in the days of *Aſclepiades*; and no mention is made of the diſeaſe in Hippocrates, or in any preceding or contemporary writer. It is a diſtemper ſo ſingular and ſtriking in its appearances, that it could never be ſeen without leaving the deepeſt impreſſion upon the mind; and the ſilence of Hippocrates is a ſtrong preſumption that it did not exiſt in his time. It has been alledged that Homer was acquainted with the diſeaſe, becauſe he uſes the word Λυσσώ; but it is evident that he means to expreſs the anger or fury of a dog from paſſion, and not from diſeaſe; though the term was very naturally afterwards applied to the diſeaſe. A ſimilar derivation is to be obſerved in the French name of *rage.* Ariſtotle * is the firſt writer who expreſsly mentions the diſeaſe. He ſays that all animals beſides man, were infected by the bite of a mad dog; and that it proved fatal to every animal except man. The imperfect ſtate of his knowledge is a proof, that in his time the diſeaſe was recent. It has been remarked before, that a ſmall proportion only of thoſe of the human ſpecies who are bitten, are ſeized with the diſeaſe; and Ariſtotle might have ſeen ſeveral who received no injury from the bite of a mad dog. Again, the diſeaſe not appearing till long after the infection, and the ſymptoms in man differing greatly from thoſe in the dog kind, in which there is no difficulty or averſion to ſwallow either liquids or ſolids, ſome time might elapſe, and many caſes of hydrophobia occur, before it was referred to its true ſource. But a period of no great length of time muſt have been ſufficient to clear up all theſe doubtful points; and when we advert to the total ſilence of Hippocrates, the imperfect knowledge of Ariſtotle, and the familiar manner in which ſubſequent writers ſpeak of the diſeaſe, we are tempted to conclude that canine madneſs did not exiſt in Greece till a ſhort time before the days of Ariſtotle, that is, about two thouſand one hundred years ago.

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* De Hiſtor. Animal. lib. viii. cap. 22.

XVIII. *Some Obſervations on Ulcers.* By EVERARD HOME, *Eſq; F.R.S. Aſſiſtant Surgeon to St. George's Hoſpital.*

As the management of ulcers is one of the moſt common offices of ſurgery, the applications employed for that purpoſe, are almoſt without number. Yet the uſual modes of treating ſuch complaints, when ſituated on the lower extremities, have been ſo unſucceſsful, that they are regarded, not unjuſtly, as the opprobrium of the art.

Ulcers generally conſidered, vary in many reſpects, according to the conſtitution of the patient, and the parts on which they are ſituated; the modes of treating them can, therefore, be collected only from a field of practice of conſiderable extent, and form a topic of enquiry, much too various and important, for the limits of this paper. It is not my intention at preſent to enter minutely into the ſubject; a few obſervations on the treatment of ſuch ſores as are of no ſpecific nature, but from weakneſs, indolence, or long habit, have no diſpoſition to ſkin, will be all that I ſhall now bring forward.

The uſual mode of treating ulcers of this kind is by ſtimulating, or by ſedative applications; when neither of theſe are thought neceſſary, dry lint is commonly applied to the ſurface of the ſore, to abſorb the matter, and ſome unctuous covering is laid over it, to prevent evaporation, and more readily admit of the removal of the dreſſings.

Stimulating medicines are commonly mixed up with ſome of the expreſſed oils, and applied in the form of an ointment; but thoſe applications that are intended to act as ſedatives, are uſually combined with water, and made uſe of in the form of fomentations and poultices.

Both theſe modes of applying medicines to an ulcer, experience has proved to be improper as a general practice. Unctuous dreſſings are found not unfrequently to diſagree with the ſurrounding ſkin, and, when brought in contact with the ſurface of an ulcer, become extremely irritating. Watery dreſſings, from the weight which is neceſſary for their conſtant application, are in many inſtances both inconvenient and hurtful; and, when ong continued, give the granulations a diſpotion to be luxuriant and flabby.

From the knowledge of theſe facts, which are I belive ſufficiently eſtabliſhed, ſurgeons have, for ſome years paſt, applied their minds to ſtrike out ſome improvement; and various ſubſtances in the form of powder have been propoſed, as ſubſtitutes for the common dreſſings.

In the courſe of theſe laſt fourteen years, I have had frequent opportunities of attending to caſes of this kind, and have made uſe of a variety of ſubſtances, in different forms, as external applications. I have not only tried thoſe powders recommended by others, but have uſed ſome which had not before been thought of. Of the latter kind, a few appear to me to deſerve particular attention; my experiments and obſervations upon which, I have thrown together in this paper.

As the application of powders to ulcers in general is a practice not yet much known, it may not be improper firſt to mention the reſult of my experiments on ſuch ſubſtances as have not been found to anſwer, that others may not loſe time in uſeleſs trials.

Tartar emetic, in the form of powder, has been recommended, and I have uſed it in a variety of inſtances; in ſome it produced no viſible effects, and in others ſuch as evidently were unfavourable; nor in any caſe could I effect a cure, by perſiſting in the uſe of it.

Chalk has had its advocates, but experience does not authoriſe me to give my teſtimony in its favour; it may anſwer in ſuperficial ſores, where nothing in fact is neceſſary to be done. This powder, when firſt applied, does not ſeem to produce the ſmalleſt irritation; but if it be long continued, the ſore in general, although not alaways, becomes foul.

Plaiſter of paris is an application I was led to make uſe of ſeveral years ago, from an idea that it would abſorb the matter of a ſuperficial ſore as faſt as it was ſecreted, and form a cruſt, which, ſimilar to a ſcab, would induce the parts underneath to ſkin over; experience, however, proves that this is not the caſe, and the edges of the ſore, in many inſtances, are much irritated by this application.

Lapis caliminaris, in the form of powder, has had a character with ſeveral ſurgeons for having the power of diſpoſing ſuperficial ſores to ſkin over; I therefore thought it deſerving of a trial. I uſed it in a great many inſtances, and no one circumſtance has occurred from which I could conclude in favour of its effiacy.

That each of theſe four ſubſtances has been found, in particular inſtances, to ſucceed, I am very ready to believe; but, as far as my own obſervations lead me, I am inclined to think that they are not to be ſet down as applications on which dependence can be placed in the cure of ulcers.

The uſe of theſe mineral ſubſtances in powder having proved unſucceſsful, I was led to conſider how far the vegetable medicines could be applied in that form with advantage; and I was encouraged in this enquiry from having, in many inſtances, experienced the good effects of the carrot and caſſada poultices *, which are nothing more than the powder of thoſe roots in a moiſt ſtate.

During the laſt war, I was eight months in the iſland of St. Lucia, in the Weſt-Indies, where ulcers on the the legs of the worſt kind were exceedingly numerous; and the application from which they appeared to derive the greateſt benefit, was the caſſada-root grated into a powder, and moiſtened with

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* The caſſada is the ſatropha manihot of Linnæus.

water. It did not at that time occur to me, that it might be uſed in a dry ſtate.

As rhubarb has conſiderable powers in ſtimulating the villous internal ſurface of the ſtomach and inteſtines, which in appearance is not very diſſimilar to that of a granulating ſore, I made choice of this powder as an external application.

I began my experiments by covering the ſurfaces of ſeveral ulcers with the powder of rhubarb, very thinly ſpread over them, and afterwards dreſſing them with a pledget of common ointment. Theſe applications were renewed once in twenty-four hours. Under this treatment the ſores mended for ſeveral days, and then ſeemed to be at a ſtand, although they retained a healing appearance. This circumſtance was not readily accounted for, but as the pledget was exceedingly greaſy, and came in contact with the edges, and many other parts of the ſore, it was attributed to that cauſe, and a piece of lint was therefore interpoſed between the powder and the unctuous covering, after which the granulations became diſpoſed to form ſkin, and the ſores were gradually healed.

Encouraged by theſe trials, I made uſe of this powder under a variety of circumſtances, with a ſimilar reſult. In ſome caſes it appeared, after being continued ſome time, to loſe its effect upon the ulcer. But even in theſe inſtances, the applying it twice in the ſame day was ſufficient to reſtore the firſt effect. In other caſes the rhubarb ſeemed to be too violent an application, in ſome degree diſpoſing the granulations to ulcerate; this effect was however prevented, by adding to it the powder of opium, in the proportion of a drachm to an ounce. By theſe means the application of this medicine may be adapted to the degree of indolence or irritability in the ſore to which it is applied.

Having aſcertained the effects of what is commonly called Indian rhubarb, I made comparative trials with that imported from Turkey, but could not diſcover any difference that appeared material. Rhubarb in the form of * tincture did not by any means produce ſimilar effects; on the contrary, the ſores to which it was applied, when irritable in their

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* The tincture uſed in theſe experiments, was made by pouring proof ſpirits upon the powder of rhubarb, and, after letting it remain three or four days without adding any other ingredient, the tincture was ſeparated for uſe.

nature, put on a foul appearance; when indolent they were leſs affected, but did not acquire the ſame healing diſpoſition that the powder uſually produced.

After confining my experiments to rhubarb, and having this ſucceſs, I was naturally led to try the effects of other vegetable powders. It ſeemed neceſſary to determine whether this healing property was peculiar to rhubarb, or, if it be in other vegetable ſubſtances, in which of theſe it exiſted in moſt perfection.

With this view I ſelected the following powders, as proper ſubjects for my experiments, and ſhall ſtate, in as few words as poſſible, the reſult of my obſervations upon them.

Ipecacuanha appeared to irritate the granulations to which it was applied, giving the ſore a foul appearance *.

The flour of muſtard, applied by itſelf, not only irritated the granulations, but excoriated the ſurrounding ſkin; to prevent theſe effects, it was mixed with the farina of lintſeed meal in different proportions. Thus

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* Equal parts of ipecacuanha and rhubarb make an uſeful mild eſcharotic for deſtroying warts.

weakened I applied it to ſeveral ſores, but did not find its general effects ſufficiently favourable to induce me to continue this application.

The gentian and chamomile were uſed ſeparately, in a variety of caſes. The ſores under this treatment went on tolerably well, and the granulations put on a florid and healing appearance; but the proceſs of forming new ſkin was extremely ſlow, when compared with the effects of rhubarb. Comparative trials of theſe powders with rhubarb on the ſame ſore, afforded ſufficient evidence of the inferiority of their powers in diſpoſing ulcers to heal.

The powder of the columba-root appears, from its effects, to bear a greater reſemblance to the rhubarb than any of the other ſubſtances which were made trial of. In ſeveral caſes where the ſores had become ſtationary under the application of the rhubarb, changing it for the columba produced a diſpoſition to heal, which continued till the ſores got well. This certainly is in favour of the columba, but is no proof of its ſuperiority, ſince all medicines loſe their effects from being long continued, and, when that happens, ſimply leaving them off may produce a cure, or diſpoſe the parts to get well under applications which before had failed.

My experience of the effects of this medicine being leſs extenſive than of thoſe of rhubarb, I am unwilling to ſay more reſpecting it; but in uſing columba, the granulations ſometimes riſe above the ſurface, a circumſtance which does not occur when the rhubarb is uſed.

It will hardly be neceſſary to mention here the difficulties which muſt occur to every medical practitioner, in aſcertaining the effects of a medicine; the only ſatisfactory mode of doing which, is to compare a number of reſults, obtained under different circumſtances, deducing our concluſions from the aggregate.

In determining the effects of local applications, the ſurgeon is often miſled by ſome circumſtances which are not, I believe, ſufficiently attended to; theſe, therefore, I ſhall briefly mention.

It ſhould be obſerved, in the firſt place, that almoſt all ſores appear for a time to receive benefit ſimply from a change in the dreſſing, although the new application be by no means better than the former. In the ſecond, that all local applications loſe their effects upon a ſore, whatever they may be, after long continuance.

The circumſtances of the patient are alſo to be taken into the account, before we can well aſcertain the effects of our treatment. A man who has been ſuffering all the diſtreſſes of poverty, when taken into an hoſpital, where he is kept quiet, and well taken care of, will mend under any treatment; and in many inſtances would get well without the aſſiſtance of medicine. In all ulcers, on the lower extremities particularly, reſt alone is of the utmoſt benefit. To theſe and ſimilar circumſtances, attention muſt be ſtrictly paid, with due allowance for their natural effects, in all experiments that are attempted on this ſubject.

Having given a general view of the effects of rhubarb as an application to ulcers, and compared them with thoſe of other vegetable and mineral powders; I ſhall add only a few obſervations upon the particular changes which moſt commonly occur in ſores while under this treatment.

In mentioning rhubarb as a dreſſing, it is not meant to bring it forward as one that will in every caſe ſucceed; but only to introduce it into practice, as a mode of treatment ſuperior to ſome now in uſe: as one which will be found not unfrequently to give the granulations a healing diſpoſition, and, when they have arrived at the external ſurface, diſpoſe them to form ſkin.

In the courſe of the laſt twelve months I have uſed this dreſſing almoſt conſtantly in St. George's Hoſpital, in a large infirmary, where ulcers in the legs are very numerous, and in private practice. From the different caſes which have occurred in theſe ſituations, I have been led to make the following remarks upon the effects of this medicine.

When the rhubarb is firſt applied, the ſore is commonly more painful than it was before, and the whole ſurface becomes of an uniform red colour. In foul ulcers, where the matter is principally compoſed of coagulable lymph, adhering to the ſurface, a change is produced in the diſcharge; it becomes more like true pus, which ſeparates from the ſurface, and expoſes the granulations to our view.

In ſores where the granulations are large, ſpongy, and ſemitranſparent, under this treatment they become ſmall, firm, very pointed at the ſurface, and of a florid colour; they are alſo ſenſible to the touch, making the ſore extremely tender.

The granulations, as ſoon as they have riſen to the level of the ſkin, ſeem to have a ſtop put to their growth; and thoſe that are next to the ſurrounding cutis loſe their irregular pointed appearance, become ſmooth, ſhining, and of a more florid colour than the reſt of the ſore. In this way a margin is formed, about one-eighth of an inch broad, which is afterwards covered by a cuticle, contracting the ſize of the ſore; and this proceſs goes on till the whole is ſkinned over.

This circumſtance of the granulations becoming ſtationary when arrived at the level of the ſkin, is ſo general an effect, that in an hundred caſes, many of theſe ſores nearly equal to ſix inches ſquare, no one inſtance to the contrary has occurred.

When the ſore is beginning to heal, or has in part ſkinned over, the powder adheres firmly to the edges, and ſometimes to the ſurface of the granulations, forming a cruſt over theſe parts. This ſhould be carefully removed at each dreſſing, as it confines the matter, which is often productive of miſchief, and always retards the progreſs of the cure.

From theſe facts, which have been aſcertained by experiments too frequently repeated to admit of much fallacy, I venture to recommend the rhubarb as an application, which will be found uſeful in the treatment of ulcers; leaving it to a further and more complete experience to determine the exact place it is to hold among the applications now in uſe.

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